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EDUCABILITY OF BINOCULAR MOTOR PATTERNS

BY E. A. BOTT, G. G. BROWN AND L. H. COHEN[1]

In normal vision convergence and divergence of the eyes comprise a particular set of motor patterns which afford fusion of the monocular fields. It is the purpose of this study to report upon an experiment designed to ascertain whether the eyes will learn a different set of motor patterns in order to achieve binocular fusion when the direction of rays from the fixated object to the eyes is modified in a special manner. If it can be shown that under a special mode of stimulation the eyes will develop a special form of coördination to facilitate the maintenance of binocular single vision, this would lend support, in theory, to the view that highly developed patterns such as those of normal convergence and divergence are perhaps similarly derived, *i.e.*, are an adaptation resulting from persistent practice under the particular conditions of stimulation that the natural world affords us.

We shall discuss first certain features of normal eye-movements as contrasted with the patterns to be experimentally induced, then the procedure for developing the latter, and finally the results obtained.

First, the eye as a motor organ permits the line of regard to be moved through a considerable angular range in all directions about the primary position; and the two eyes, functioning as a single receptor-effector system, tend to fixate a common point or limited area, the net conscious result of

[1] From the psychological laboratories of Yale University and the University of Toronto. The authors desire to acknowledge the helpful suggestions and criticisms of Professor R. Dodge of Yale and Dr. A. E. MacDonald of the Department of Ophthalmology of the University of Toronto, but assume responsibility for the views expressed.

this being that the fields are fused. This tendency to maintain binocular fixation under all possible circumstances seems to be one of the most inherent properties of the visual system in man, and in the motor process involved one eye usually plays a master rôle. The point binocularly fixated may have its locus at any angular position in the subject's effective binocular field and at any distance beyond his near point of binocular vision, *i.e.* approximately 8 cm. The muscular integration required to maintain this unique relationship of the eyes (aside from the process of accommodation) is obviously very complex, necessitating, on occasion, rotation of the two globes to different extents in three dimensions from the primary position of regard.

Of this complex adjustment the movements of convergence and divergence are the most prominent overt features; but no less important is the withholding of other ocular movements which, if they occurred, would frustrate successful fusion by tending to produce double vision. Such inhibitory control is evidenced in the fact that movements of convergence and divergence are normally closely restricted within the fixation plane [2] defined by the interocular axis and the natural object fixated, with more or less rigid inhibition of anomalies of movement such as hypophoria or hyperphoria on the part of either eye. That is to say, the lines of regard can voluntarily be turned toward or away from one another within any fixation plane but cannot in a like way each be operated in separate planes lying in that axis or intersecting it. Diplopia or some compensation therefor is the price we pay for failure to conform with this particular requirement of nature.

The anatomical placement of the eyes in a trans-median plane may be accepted as one of the basic facts underlying this type of motor pattern as our means for securing binocular fixation. Whether the mechanisms of ocular muscle-action

[2] The coördination is not confined literally to this geometric plane but deviation from the fixation plane is small and resists amplification. This inhibition of movements extraneous to fixation Helmholtz considers "is not an obligatory anatomical mechanism, but is rather something which can be altered by the mere influence of our own volition; . . . the only restriction consists in controlling the intent of our will, so far as its sole purpose is distinct and single vision." *Physiol. Opt.* (Engl. tran.), Vol. III, p. 59.

and innervation are capable of this coördination alone (which Helmholtz seems to doubt) or can be reëducated to utilize other patterns for fixation purposes is the question at issue, and the answer should be sought by experiment. In order to do this one must accept (1) our endowment of the bi-lateral arrangement, (2) the fact that we already possess the function of convergence and divergence within fixation planes that lie either upward or downward from the primary position, and (3) the circumstance that we cannot (or can only to a slight extent) coördinate the eyes otherwise; for example, vertically, looking upward with one and downward with the other—as would be the 'natural' coördination for fixation purposes were the eyes set in the median rather than in the trans-median plane of the head. But while we cannot conveniently interfere by direct experiment with the ocular equipment, we can safely and easily deal with the external stimulus; that is, we can control the direction of the incident light and then allow the eyes to respond with such movements of fixation as they will.

Our experiment aimed so to control stimulation that binocular fixation could not be obtained without vertical coördination, *i.e.* without employing a pattern of antagonistic superduction and subduction on the part of the eyes respectively. While this might be described as an induced hyper- or hypo-phoria, it is not strictly so because the latter terms presume that the fixation of one eye is adequate in a sense that may be considered normal while the direction of the other is inadequate as regards fixation through being unduly elevated or depressed. In our experiment, on the contrary, the fixation of either eye is as adequate as that of the other, the visual axes being duly fixated on a common object (or virtual image thereof), but in order so to fixate vertical deviation is necessary. In a circumstance of this unusual sort approach by the visual axes might perhaps be termed "conphoria" with the meaning of vertical convergence, and their corresponding movement of separation, "diphoria." Owing, however, to bilateral placement of the orbits, convergence and divergence in the ordinary sense will here still be required (in addition to

conphoria and diphoria) in order to satisfy binocular fixation.

Practice of such a coördination had of necessity to be relatively brief and transitory;[3] and interference from previously established motor patterns and from reversion to the latter between practice periods could not be avoided. Under such conditions it was felt that even limited indications of progressive facilitation of the new pattern might be suggestive of greater possibilities in this direction if constant practice were feasible.

Our immediate aim was two-fold; to measure, first, to what extent the eyes without special training are capable of attaining binocular fixation in terms of a pattern involving primarily antagonistic sub- and super-duction, and, secondly, to what extent this ability may be increased by systematic practice based on sound principles of reëducation. The investigation was conducted simultaneously at Yale and Toronto under substantially identical conditions, the results with five and three observers respectively being in close agreement.

The apparatus consisted of an objective field and a flexible prism stereoscope arranged to permit of vertical as well as horizontal displacement of the visual field of either eye by manual operation of the respective prisms. The field was a sheet of white cardboard about 60 cm square with a black ink line 0.5 mm broad drawn horizontally across its center and a short vertical cross line 2 mm long to mark the middle of the field; indirect illumination was used. Toward either side of the field a scale in tenth-degrees (of visual angle) was shown extending for 6 degrees[5] upward and also downward from the center line as zero. These scales were sufficiently peripheral and inconspicuous not to interfere with the subjects' attempts to maintain binocular fusion of the line in the mid-field. They furnished a convenient means of measuring at any stage of the experiment the amount of vertical displacement of either visual field, viz., by sighting with the appropriate eye the point at which the virtual image of the center line as seen through the refracting prism intercepted the scale on the field as seen simultaneously past the rim of the prism, as in Fig. 1.

---

[3] The eye movements which normally promote binocular fixation being continuously practised through practically the whole of one's waking life are in point of uninterrupted training probably the most highly favoured of voluntary muscular exercises.

[4] E. A. Bott & S. N. F. Chant, this *Journal*, 1925, 8, 133–148.

[5] Based on Helmholtz's claim: "In the vertical plane likewise I have no difficulty in producing deviations of the two eyes amounting to six degrees up and down," *op. cit.*, p. 59. We found no subject who could much exceed three degrees even after training.

The observer was comfortably seated, head steadied against temple pads, eyes level with the mid-field and at one meter distance, prisms immediately in front of the eyes respectively. To measure the amount of displacement of either visual field the subject leaned back a few inches and sighted monocularly through and past a refracting

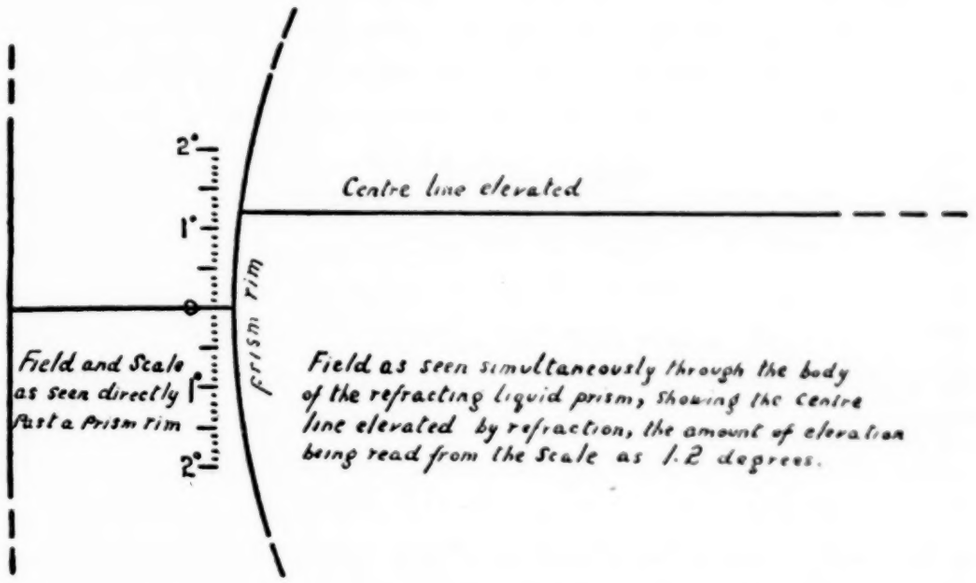
2°
1°
Centre line elevated
prism rim
Field and Scale as seen directly past a Prism rim
Field as seen simultaneously through the body of the refracting liquid prism, showing the centre line elevated by refraction, the amount of elevation being read from the Scale as 1.2 degrees.
2°

FIG. 1. Method of reading monocularly the angular displacement of a visual field

prism as described above. During binocular observation, when the faces of each prism were arranged parallel, the subject perceived the full field as he would without the use of prisms. Care was taken to avoid lateral displacement of either visual field so that the convergence normally required for fixation would not be affected.

As a first point of procedure the faces of one prism were left parallel (with its field therefore unmoved) whilst the refracting angle of the other prism was so modified that the field it presented would be visually elevated or depressed to any desired extent. If, for instance, beginning with all prism faces parallel and both eyes fixating the middle of the line, the left field be not disturbed but the right be progressively elevated, it is clear that so long as the left eye continues to hold its original fixation on the stationary line and the right eye rises to maintain fixation on its rising line, the conditions of binocular fixation will be preserved and the line will therefore continue to be seen single. Moreover, under these and analogous experimental conditions unitary vision is *only* to be had by voluntarily directing the lines of regard so that they fixate the same subject matter by looking at different

levels [6]—a coördination which nature not only spares us but would regularly punish with double vision. When tested, the subjects all showed some slight ability to coördinate the eyes in this manner for purposes of binocular fixation.

Measurement of this ability was taken in terms of that angular separation of the fields at which the eyes just failed to maintain their respective fixations as evidenced by the first appearance of doubling of the ink line. This constituted the 'break' threshold. Unequivocal criteria for the break threshold are difficult to define. As the threshold is neared the line may thicken before it divides, or may lose its sharpness; sometimes the edges become slightly billowed giving a rope-like appearance; again the line may separate momentarily and at once re-fuse, repeating this several times before finally remaining combined or separated; on some occasions rivalry occurs leaving the line single at the center and doubled in the periphery. A stable result showing two intact lines with just appreciable clearness between them was accepted as a 'break.' The determination of a threshold required usually less than one minute. Having noted a break, the angular displacement of one field from the other that had been required to produce it was then read by the subject directly off the scale on the screen.

The second step of procedure was to begin beyond the limen of the break so that the lines appeared grossly separated and then to reduce their separation by slowly moving one visual field prismatically toward the other until the lines fused. This was the 'make' threshold. Here the final stage of approach regularly ended in a spurt of movement which brought the fields into fusion with a snap in the manner well known of normal convergence-fixation when under experimental control. Once fusion was made it could not then be disrupted by voluntary effort [7] and interference by prismatic means would

[6] Analogous results were obtained when both visual fields were simultaneously displaced in opposite directions and when only one field was prismatically moved. The latter technique was adopted throughout for the sake of simplicity of operation and measurement.

[7] Persistent attempts might render voluntary dissociation possible in the case of vertical coördination as it does for many subjects in respect of voluntary control over near or far convergence, but this was not undertaken.

not dissociate the fields unless continued to the break level. The make threshold was considerably less in degrees than the break at the outset of the experiment; it increased with training but less rapidly than the other, so that the difference between these thresholds became progressively more marked.

A third type of critical reading, the 'seek' threshold, was taken by the Yale subjects. This is a special feature that came to light relating to the break threshold and may or may not be noteworthy. The phenomenon is as follows. If after arranging a break and pausing to take the reading thereof the subject then tries to re-fuse the lines, he finds it impossible to do so; they stand far apart but not the full amount that the fields are separated by the refraction of the prism. That is to say, the eyes, having held the lines in fusion until the fields were deviated to the point of break, cannot recover this fusion once it is completely relinquished; nor on the other hand do they revert to their normal pattern of horizontal equilibrium. In quest of fusion they deviate and *seek* a position of balance which is a compromise between horizontal equilibrium and that new coördination, the break position, which would be the goal that would permit fusion. Manifestly in thus seeking an equilibrium of fixation at least one of the eyes has moved from the position it previously held; moreover a particular and fairly stable vertical coördination is thereby established. Subjectively, however, one cannot say which eye it is that has ceased to fixate its line or whether both have; eye dominance in the sense accepted by Parson [8] rather than any detail of our procedure, may be a factor in determining how this new position of balance comes about.

The amount that the eyes thus deviate when they seek to fuse stimuli set at the break position can be indirectly ascertained as follows. The total deviation of the stimuli may be regarded as falling into two portions, viz. that partial deviation which the visual axes accomplish and the remaining portion which they fail to accomplish. The latter is directly perceptible to the subject as the visible separation of the fields (previously commented on) and this may be read binocularly

[8] B. S. Parson, *Left-Handedness*, Macmillan, 1924.

in degrees through the prisms, using the scale of one field and the line of the other. This direct angular reading expresses the 'seek' threshold in subjective terms, being the appearance (*i.e.*, apparent separation) of the unfused fields; this value we term the 'seek appearance.' The remaining portion of the total stimulus deviation (*i.e.*, the accompanying axial deviation of the eyes) can then be computed as the arithmetic difference between 'seek appearance' and that particular break threshold that is used as stimulus setting. This indirect reading which expresses the vertical deviation that the eyes objectively assume when seeking (unsuccessfully) to accomplish binocular fusion we term the 'seek position.'[9] These two values are alternative as defining the seek threshold; when considering questions of eye coördination 'seek position' is the proper one to use.

When attention was directed to this phenomenon of the seek threshold it was noted (1) that the value for 'seek appearance' was always considerably less than the break threshold but was not equivalent to the make threshold, (2) that while under observation 'seek appearance' remained fairly stable, any modification in it being mainly involuntary, (3) that as the break threshold increased with training the 'seek appearance' increased at about the same rate which implies that the 'seek position' of the eyes increased but little if any, (4) on working over all the threshold data from the standpoint of a study in learning it was found (as will be shown) that a fairly constant functional relationship obtains throughout between the values for the break, seek and make positions. What significance, if any, attaches to this fact we have not concluded.

Records for all three thresholds were obtained under four conditions, viz., for upward and downward movement of either eye with the opposite field unmoved. Three successive readings were averaged for a record under each of these

[9] For example, if the break threshold has been found to be 1.6 degrees and the eyes are unable to recover this fusion after it is relinquished (the fields remaining apart by an amount seen on the scale to be, let us say, 1.1 degrees), then we conclude that the eyes are standing in vertical deviation, namely, to the extent of 0.5 degrees. Briefly stated, stimulus deviation less 'seek appearance' equals 'seek position.'

conditions. Eight subjects gave measurements of the make and break and five of all three thresholds. The subjects had daily sittings (seven per week) of from 25 to 30 minutes duration including short rest-pauses. Threshold readings were recorded at the beginning of the sittings on every third day, there being at least seven such occasions for all subjects. The remainder of the periods when these records were taken and the whole of the intervening practice periods were devoted to reëducation drill on the new motor patterns.

This daily drill was arranged in accordance with the accepted principles of reëducation of muscle-function; first, systematic practice upon simple and specific functions, motivating the subject strongly to achieve a goal just beyond his then capacity, and progressively extending this objective according as his accomplishment and attitude warranted. The functions so used were the make and break limens above described, no records being taken during a routine practice. Secondly, free exercise, involving various combinations of the functions undergoing reëducation, and others. For our subjects this required visual exploration of a field more complicated than our screen in order that the new pattern of vertical binocular fixation might be adequately exercised. For this purpose a three-dimensional field was used in front of the cardboard screen, consisting of a large cork with several spikes 6 to 8 inches in length radiating laterally and forwards. When this 'pin-cushion' configuration was prismatically depressed or elevated for one eye to the verge of doubling, visual exploration of its various parts fully exercised the new fixation pattern in all its gradations and in a manner comparable with the normal exercise of convergence when the eyes freely view the world of nature. With our pin-cushion the motor pattern was a complication of convergence, divergence, conphoria and diphoria, as previously explained, and the exercise as here described required facile adjustment in this complex pattern in order that unitary vision should be continuously maintained. As in all reëducation of muscle, we believe that this type of general practice contributed materially to the outcome of learning; the measurement of improve-

ment, however, was taken exclusively in terms of the simpler functions [10] and a two-dimensional field.

Ordinary precautions against gross errors were maintained. The subjects' eyes were tested and corrections used. The displacement readings on the field scale can be accepted to the nearest twentieth of a degree. The direct method of reading displacement of the vision fields was found to be more accurate and convenient than by calibrating the refraction of the prisms and computing the displacement. Certain details of procedure were tried in various ways; control of the prisms by the subject or by the experimenter, moving a field steadily or by small increments, viewing the fields continuously or momentarily occluding the field that moved, etc. The former of these alternatives was in each case deemed preferable.

During manipulation of the fields, but while they were yet fused, subjects frequently volunteered comments concerning vague movement of the field as a whole without being able to specify the direction of this movement with respect to themselves, or to say which eye was being displaced or whether up or down. No exhaustive introspective study was attempted. Kinæsthesis by itself seemed very inadequate as a means of detecting the type or amount of experimental control and subjects were encouraged to ignore it. Strain was appreciable during voluntary effort to accomplish fusion under the new coördination but was not excessive or cumulative unless sittings were too prolonged. No ill effects or after effects of the experiment have been heard of. Considering the persistent practice that adult ocular-motor patterns enjoy, the amount of practice that this new one received was absolutely and relatively very small, nevertheless the data as given below suggest appreciable if not characteristic learning.

Results

The improvement of the make- and break-thresholds for eight subjects is indicated in Fig. 2. The solid lines represent the mean results for each threshold, obtained by averaging the readings for the four positions of the eyes (supra, p. 8) at each trial. The dotted and broken lines similarly represent the accomplishment of the right and left eyes separately, *e.g.*, the average range that the right eye could operate upward and again downward with the left unmoved and *vice versa*, the solid lines being the unweighted average of this performance by the respective eyes. All but one of our eight subjects

[10] In this experiment, as well as generally, the authors are somewhat sceptical of whether simple limens, which usually are highly abstract functions, will necessarily reflect in their measurement the improvement that may characterize a process more complicated than the limen functions themselves. Before assuming that liminal values indicate a subject's capacity in functions involving additional variables we feel that this thesis should first be examined and proved for the case in question; and obviously this cannot be done merely by appeal to the liminal values themselves. Precaution on this score is particularly pertinent for problems in visual perception and may be more pertinent than we sometimes concede for the field of psychophysics generally.

contributed to the improvement here shown, there being rather marked individual differences. In the case of the subject who showed practically no improvement, the experi-

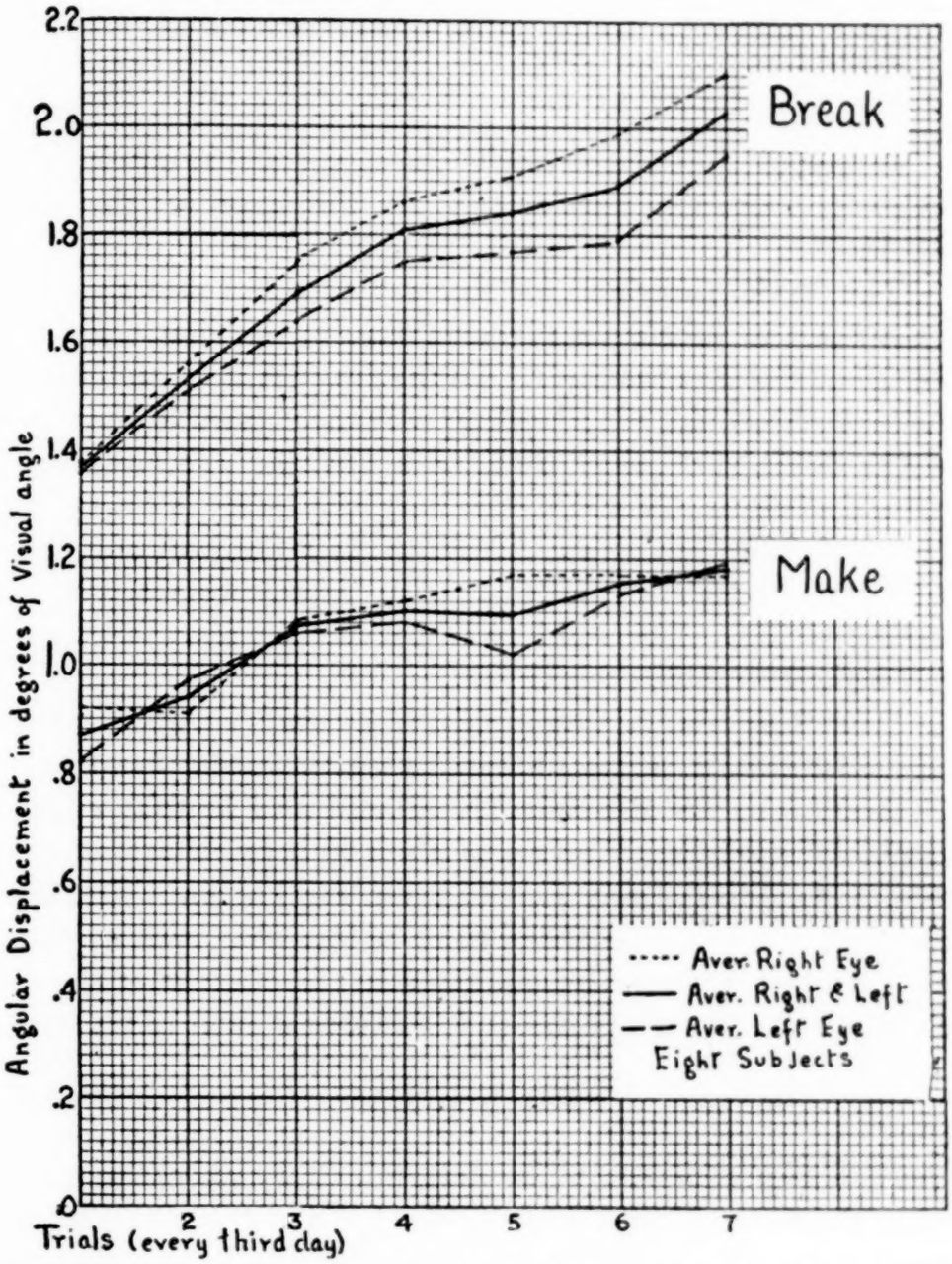

FIG. 2. Improvement in vertical deviation of the eyes measured by the break and make thresholds

menter was of the opinion that the attitude of this subject towards the reëducational task militated against his making a conscientious effort; his data are included.

The mean deviations in degrees for the above general averages (solid lines) are as follows:

	Trials						
Thresholds	1	2	3	4	5	6	7
Break	.19	.31	.30	.39	.50	.50	.56
Make	.28	.30	.26	.28	.36	.36	.42

It is seen that the variation increases as the practice and learning proceeds, particularly in the case of the break threshold.

The 'seek' threshold for five subjects and also their data for make and break are given in Fig. 3, showing simply the combined average readings. Of the alternative expressions for the seek threshold, viz., 'seek appearance' (visible separation of the fields vertically) and 'seek position' (corresponding vertical deviation of the eyes), only the latter is here shown (the dotted line) in order to indicate how the eyes coördinate with respect to the stimulus fields when presented at the break-position. The solid and broken lines show the break- and make-thresholds respectively.

From the results thus secured, as well as upon general considerations, it seems probable that these threshold values (B, M and S) are dependently related and in that event it should be possible in terms of their general relationship to predict the third if any two of them are known. Empirical examination of these data led us to believe that such a relation exists [11] and that it can be expressed to the effect that the sum of the break and seek positions is a constant ratio of the make; *i.e.* that

$$B + S = KM$$

where K is a constant to be determined by experiment. In our case by sampling the data, K of the above equation was found to be 2.64; and on this basis the predicted values, for example, of M (with B and S known) agree closely with all the observed values of M. The prediction error is within about 2 per cent. of the gross values of M throughout, which

[11] The suggestion of such a constant relationship and of its possible significance we owe to Dr. MacDonald.

may be considered to be within the range of experimental error. In Fig. 3 these predicted values for the make-threshold are shown by a cross in conjunction with each observed reading (the broken line).

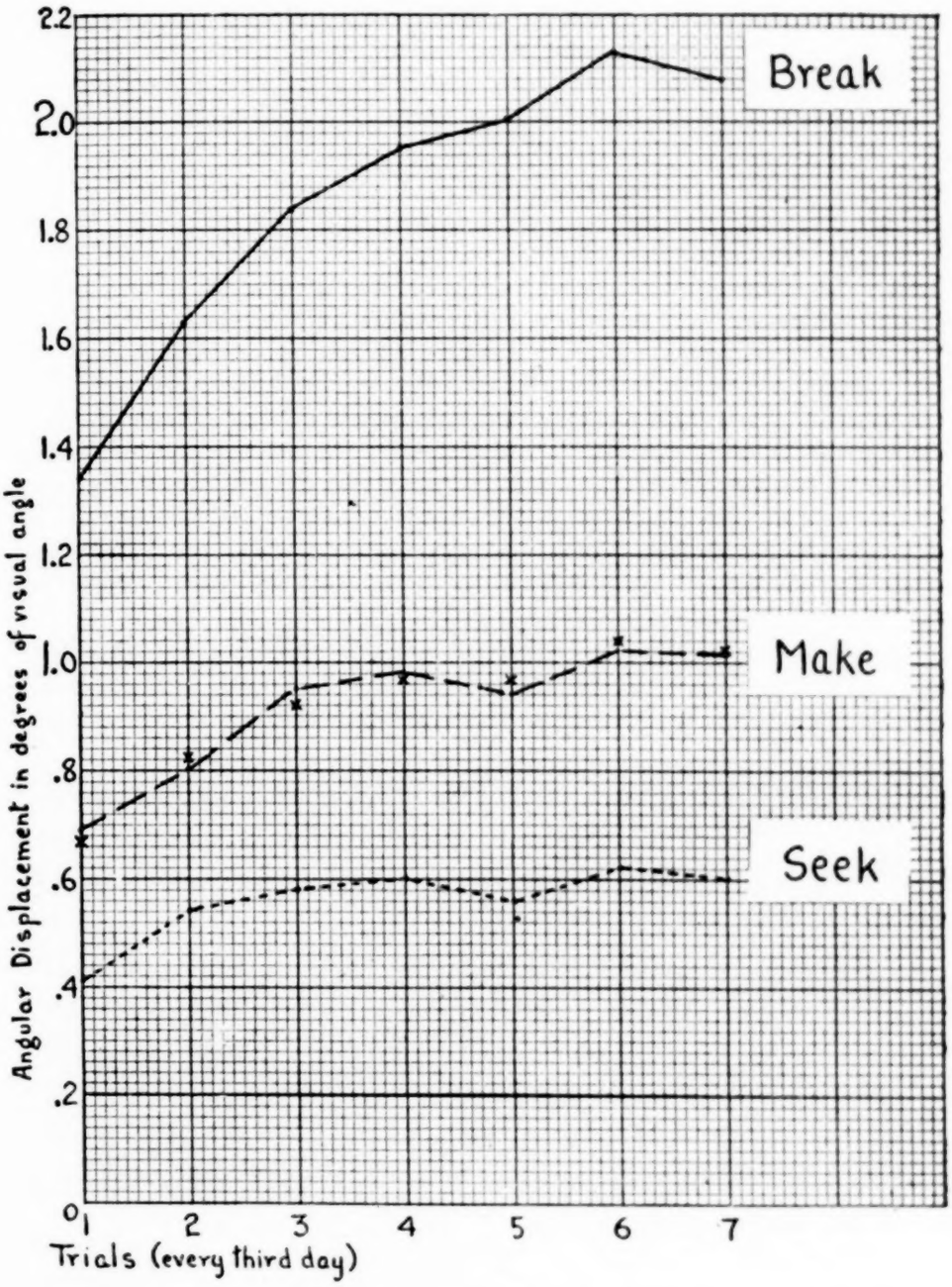

FIG. 3. Relationship of the Seek threshold to the Make and Break thresholds

Whether the above relation of dependence warrants incorporation into a generalization, how best to express it, and how it should be interpreted are points that invite

further experimentation. It will be appreciated, for instance, that the particular seek-reading which we have considered is a special case falling within a wide range of possible *S* values of which full account should be taken in formulating a broad generalization. To illustrate: If a single objective field is binocularly presented, the eyes will fixate it in horizontal equilibrium, *S* then having zero value; if, however, one of the visual fields be presented elevated an amount equivalent to the break-reading the eye whose field it is will seek to fixate it and will rise to the position *S* that we have noted. But there are other possibilities which extend to limiting values for this seek position: thus if the elevated field be shown with a deviation greater than the break-value, the eye will still be motivated to fixate it but less strongly, *i.e.* *S* is decreased; and when the field is elevated extremely the eye will ultimately cease to seek it and revert toward horizontal equilibrium (*S* decreasing to zero). On the other hand, if the field is given an elevation less than the value of the break-threshold the eye is increasingly motivated to secure it in fusion and rises to a seek position greater than that which we observed when the stimulus was set at the break value, *i.e.* *S* increases. As the field elevation is still further decreased the seek position of the eye ultimately increases until the field and the visual axis come into conjunction and give binocular fusion, this position constituting the make-threshold. Depending therefore on the stimulation conditions as regards vertical deviation of the fields (and independent of our training problem), the value of the seek-position, *S*, may vary from a minimum (horizontal equilibrium) to a maximum (the make-threshold). Within this range that seek-position which offers for fields separated an amount equivalent to the break-threshold is only one case and by no means represents the whole story of the seeking performance of the eyes or the 'tendency towards binocular fusion.' It is questionable, for instance, whether for theoretical or explanatory purposes we are justified in regarding 'fusion tendency' as an entity that controls eye-movement being now inhibited or again facilitated by other integral forces. More information upon the facts concerning the seeking movements of the eyes preliminary to and contributory to binocular fusion will first be required.

The following points may be summarized from our facts.

1. There is appreciable and consistent learning of vertical coördination indicated by the break- and make-thresholds and possibly by the seek-threshold.

2. Ability to sustain the new pattern of vertical fixation (to the point of 'break') whilst binocular fusion remains an accomplished fact is greater than ability to 'make' the pattern when fusion is lacking but is strongly motivated. The former capacity is, under our conditions, initially about half as great again as the latter, and increases at approximately twice the rate of the other throughout the period of training.

3. The mean deviation (of individuals as well as of the group) increases for all thresholds as the learning proceeds, *i.e.* the new pattern is facilitated from the outset but in a spasmodic manner and habituation is slow.

4. Of the seek-threshold, 'seek-appearance' was not greatly in excess of the make-threshold at the outset of the experiment but improved more rapidly than the latter, *i.e.* at a rate closely resembling that of the break-threshold; on the other hand 'seek-position' showed relatively little increase at any stage of the experiment.

5. Although the break-, make- and seek-positions are all modified by training and to very different extents, they seem always to maintain a constant mutual relationship which is expressible in the equation $B + S = KM$, with K a constant. The full significance of the seek-threshold with reference to phenomena of binocular fusion requires further exploration.

6. On the whole, the right eye predominated over the left in improvement on the new motor pattern, there being little difference between the eyes in the case of the make-threshold but consistent and increasing superiority by the right eye for the break-threshold.[12]

Conclusions

Of the three thresholds used we consider the break-threshold the most satisfactory criterion of learning, not because it showed more improvement than the others but because it represents the limit of motor adjustment within which the fields can be held fused, *i.e.* the extreme range for binocular fixation, which latter is an essential condition of single binocular vision whether under natural or experimental conditions of stimulation.

Our results are too meagre to warrant very general conclusions. They suggest that radical modifications in oculomotor patterns can be learned, or, to be more specific, we feel that the results are sufficiently positive to show that an ocular-motor pattern such as that of vertical coördination which is fairly consistently inhibited in normal vision may to some degree at least be quickly facilitated when the circumstances demand such adjustment to meet the needs of visual perception. Our findings do not indicate anything concerning

[12] All of the subjects were right-handed, and when tested by a manuscope the right eye was indicated as the master.

the extent to which such learning could be carried under intensive and long continued practice applied ontogenetically or through generations,—unless indeed one should surmise that convergence and divergence as found in normal vision is a case in point. If a pattern of vertical coördination can be facilitated by the small amount of drill here employed, it would give ground for belief that other unusual patterns (*e.g.*, those appropriate for orientations intermediate between the horizontal and vertical) could similarly be established if circumstances demanded such adaptation on the part of the organism. But Nature on the side both of anatomy and of physics prejudices our opportunities for testing this general thesis in any very conclusive manner since she has so definitely limited the conditions under which our visual process must for the greater part operate.

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BISECTION OF TONAL INTERVALS LARGER THAN AN OCTAVE

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Among the phenomenal characteristics included within the total impression set up by two tones of different frequencies, two have generally been thought to be of central importance in determining judgments of relative size of such impressions, *interval-quality* and *distance*. By interval-quality is meant the phenomenal characteristic which enables an observer, usually one of musical training, to identify two intervals which have different absolute frequencies but the same relative frequencies. The fact that similar intervals from different parts of the tonal scale are paralleled on the side of vibration-frequencies by numerical values which bear a constant ratio to one another led the early psychophysicists to assume that in the development of the musical scale art had performed a valuable experiment for the science of psychophysics.[1] By distance is meant the amount by which the limiting tones of an interval differ with respect to auditory quality. It was not until a short time before the publication of Lorenz's experiments on tonal distances[2] that it was seriously doubted whether judgments of distance and judgments of interval-quality would yield the same results. Lorenz's data seemed to indicate, however, that psychological judgments on differences in pitch-distances do not conform to Weber's law; that, on the contrary, the same harmonic intervals represent unequal differences of sensation, and that between the absolute

[1] G. Th. Fechner, *Elemente der Psychophysik* (rep.), 1888, i, 181.

[2] C. Lorenz, Untersuchungen über die Auffassung von Tondistanzen, *Phil. Stud.*, 1891, 6, 26–103. The appearance of this paper gave rise to stormy arguments between Wundt and Stumpf. An historical and critical discussion of this literature, together with complete references, may be found in Titchener, *Experimental Psychology*, 1905, Vol. II, pt. ii, 232–248.

differences of sensation and the corresponding differences in vibration-frequencies there exists an almost exact proportionality.

A common procedure in work on tonal intervals is to determine, by a suitable psychophysical method, that value of auditory stimulus which lies for sensation midway between two limiting stimuli. If the middle stimulus corresponds to the geometrical mean the equal-appearing intervals are paralleled by equal relative differences of stimulus; if it corresponds to the arithmetical mean the intervals are paralleled by equal absolute differences of stimulus. The weight of evidence, even when the practical judgment of the musician is left out of account, seems to favor equal relative differences, at least for bisections of intervals smaller than an octave. Lorenz's results constitute the striking exception, but it should be borne in mind that his conclusions have been seriously questioned, on various grounds, by Stumpf.[3] The opinions of Fechner[4] and Weber,[5] together with the construction of musical intervals, had predisposed psychophysical belief in favor of the logarithmic relation of tones. Stumpf repeatedly emphasized[6] the difficulties placed in the way of the distance-judgment by the coercive power of the musical consciousness which causes the interval-quality to obliterate the simple difference in pitch of two tones; but he was nevertheless inclined to believe, from his own observation, 'dass das gleiche Intervall mit zunehmender Tonhöhe bis etwa c^3 eine zunehmende Distanz in der Empfindung darstellt.' Such an increase in distance with rise in pitch would not, however, bring about very appreciable differences in the relative sizes of the same intervals of different pitches within the compass of a single octave. Münsterberg, in 1890, summarized certain of his experiments on tonal distance in which it appeared that musical observers made bisections at the geometrical mean and unmusical observers nearer the arithmetical mean.[7] Later experiments, by a four-tone method in which a distance a–b is compared with a distance x–y from a higher part of the scale, indicated in general that 'das Maximum der Gleichschätzung liegt zwischen dem Punkt des gleichen Schwingungsverhältnisses und dem Punkt der gleichen Schwingungsdifferenz und zwar verschiebt es sich von jenem Punkt zu diesem mit zunehmender Grösse der Normaldistanz.'[8] In 1892 Gustav Engel, professor at the Royal Academy of Music in Berlin, published[9] casual observations of his own on estimation of intervals. He found that the midpoint for all intervals smaller than an octave coincided always, in his judgment, with the geometrical mean, whereas the midpoint for intervals larger than an octave (beginning with c^2–d^2) tended to creep up above the geometrical mean. He made several trials with the larger intervals "ohne aber zu anderen Resultaten zu kommen als dem eben ausgesprochenen, dass ich bei weiten Distanzen zwischen der geometrischen Mitte und

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[3] C. Stumpf, Ueber Vergleichungen von Tondistanzen, *Zsch. f. Psychol.*, 1890, 1, 419–462.

[4] *Loc. cit.*

[5] E. H. Weber, *Tastsinn und Gemeingefühl* (ed. by Hering), 1905, 116.

[6] *Tonpsychologie*, 1883, i, 247 ff.

[7] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 1890, 3, 37 ff.

[8] *Ibid.*, 4, 170.

[9] G. Engel, Ueber Vergleichungen von Tondistanzen, *Zsch. f. Psychol.*, 1891, 2, 361–378.

den nächst gelegenen höheren Tönen schwanke." Much more recently [10] a series of experiments on estimation of tonal distances, done by the method of equal sense distances, was reported by the present writer. Three intervals were presented to the observers for bisection: 300–410 d.v., an interval slightly larger than the natural fourth; 285–427.5, the perfect fifth; and 285–510, an interval slightly larger than the tempered minor seventh. The bisections for these intervals, for musical and unmusical observers alike, occurred with striking regularity very close to the geometrical mean.

Abraham and von Hornbostel have recently reported work on tonal distance [11] which furnishes still further evidence in support of the view that equal-appearing intervals, within the range of an octave, correspond to equal vibration-ratios rather than to equal vibration-differences. An experimental arrangement was devised which, the authors believed, served to eliminate to a large extent the phenomenal character of interval-quality. The intervals to be compared, instead of being composed of two limiting tones, were made up of a large number of adjacent notes sounded simultaneously on the Appunn tonometer. The task of the observer, consequently, was to compare one *filled* distance with another. The limiting tones of these multiple clangs could not be singled out by *O*. The clangs possessed, however, a definite *Ausdehnung* (*Klangbreite*), but no interval-quality.[12] It was therefore assumed that under these experimental conditions the criterion of judgment was auditory distance and not, as has often been thought to be the case in work on intervals, the musical quality of the pairs of tones. "Die besten Ergebnisse hatten Versuche mit Klängen, die aus einer grossen Zahl nahe benachbarter Zungenklänge zusammengesetzt waren. Hier trat das qualitative Moment (Akkordqualität) überhaupt nicht mehr in Erscheinung; dennoch konnte einer als Muster vorgelegten normalen Klangbreite eine Vergleichsklangbreite mit ausserordentlicher Genauigkeit gleichgemacht werden. Auch hier hängt die phänomenale Breite von dem Verhältnis der Schwingszahlen der Ecktöne ab." [13] The numerical data are not published. The one illustration given is of the interval 692–807 (264 cents) [14] which was equated by *O* to the standard distance of 403–469 (263 cents), an interval slightly larger than the subminor third. The standard intervals varied between 133 and 263 cents, and, together with the comparison intervals, were included, as far as one can tell from the published report, within the range of a single octave. "Lässt man, was man wohl darf, die Klangbreite als Analogen der Distanz eines Tonschrittes gelten, so ist durch diese Versuche erwiesen, dass reine, von der Intervallqualität unabhängige Distanzurteile möglich sind and dass die Distanz ebenso wie die Intervallqualität von dem Verhältnis der Schwingungszahlen abhängt." [15]

Whereas, then, the evidence for bisections of intervals not exceeding an octave and for equations of pairs of intervals with-

[10] C. C. Pratt, Bisection of tonal intervals smaller than an octave, *J. Exper. Psychol.*, 1923, 6, 211–222.

[11] O. Abraham & E. M. von Hornbostel, Zur Psychologie der Tondistanz, *Zsch. f. Psychol.*, 1925, 98, 233–249.

[12] *Ibid.*, 246.

[13] *Ibid.*, 248.

[14] A cent equals an hundredth part of a tempered semitone. For a discussion of the cent and its calculation, see appendix of A. J. Ellis' translation (2d ed., 1885) of Helmholtz' *Die Lehre von den Tonempfindungen, usw.*, p. 437, art. 24, xiii, and pp. 446–457.

[15] Abraham & v. Hornbostel, *op. cit.*, 247.

in the range of an octave seems to favor the geometrical mean and equal vibration-ratios, bisections and equations, on the other hand, which exceed the limits of an octave yield less uniform results. Evidence of the latter sort is forthcoming in the present paper. The experimental data were secured in the Harvard Psychological Laboratory during the academic year 1925–26.

The observers who took part in the experiment were *A*, Dr. J. G. Beebe-Center; *B*, Dr. Helen K. Mull; *C*, Mrs. Marjory B. Pratt; and *D*, Mr. Philip Walker. All were well trained in psychological observation. *B* and *C* were musical observers and had had considerable experience observing in studies in audition. *A* and *D* were unmusical Os.

The immediate object of these experiments was to determine for a given tonal interval larger than an octave that value of auditory stimulus which for O divided the interval into two phenomenally equal parts. The experimental procedure adopted was the usual form of the method of equal-appearing intervals modified so that the data could be treated by Urban's phi-gamma process of calculation. Two limiting stimuli, constituting the components of the tonal distance to be bisected by O, were kept constant while a series of intermediate stimuli around the central region served to separate the interval into two sense distances. These two sense distances were then the objects of judgment for O. Given, *e.g.*, the stimuli *l* (lower component of the interval), *m* (middle stimulus), and *h* (higher component), in the order named, it was the task of O to judge the distance *mh* with respect to the distance *lm* in terms of the categories 'greater,' 'equal' and 'less.' To avoid the time-error the stimuli were also presented in the order *h*, *m*, *l*, in which case O judged the distance *ml* with respect to the distance *hm*. When the data were prepared for calculation the 'equal' judgments were halved between the 'greater' and 'less' judgments and the relative frequencies thus obtained subjected to Urban's phi-gamma hypothesis for finding the ideal value of the *m* stimulus which in 50 per cent. of the cases would divide the interval into two psychologically equal parts.[16] All the relative

[16] For a description of the method of equal sense distances and the process of calculation see Titchener, *op. cit.*, Vol. II, pt. i, 81–92, and F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, 1912, 24, 236–243.

frequencies for the calculation of these medians were based on fifty judgments on each pair of intervals in the ascending order, and fifty on each pair in the reverse order—a total of 9,700 judgments for the whole experiment.

The instructions to O read as follows:

"Following the 'ready' signal you will be presented with three successive tones. You are to compare the distance between the second and third tones with respect to the distance between the first and second tones. Express your judgment by one of the three categories 'greater,' 'equal' and 'less.' A judgment 'greater' or 'less' indicates that the second auditory sense distance is greater or less than the first; the judgment 'equal' means that the second auditory distance is equal to the first. Occasionally you will be asked to give either a generalized introspective characterization of your judgments on sense distances, or a complete introspective description of a given particular judgment."

All the Os had initial difficulty in carrying out these instructions. It was necessary to devote considerable time with each O in preliminary experiments at the beginning of a new interval or time-order in order to find a series of *m* stimuli which would yield the proper form of frequency-curve of judgments. Not all the Os could make use of the same set of *m* stimuli, as can be inferred from the diversity of values of the medians in the following tables. After a certain amount of practice, however, the Os settled down to the *Aufgabe* and gave their judgments with a fairly high degree of subjective assurance.

Four different intervals were used, all larger than an octave. A brief account of the results for each interval follows.

In Series I the limiting stimuli (*l* and *h*) were 192 d.v. and 448 d.v.; an interval (1468 cents) slightly larger than the major ninth. The intermediate stimuli ranged between 264 and 344, the number and size of step between them depending on the particular O. It was necessary, *e.g.*, to use seven *m* stimuli for *A* and *D*, six for *C*, and five for *B*. The geometrical mean for the interval 192–448 is 293.3, and the arithmetical mean 320. In Table I will be found the averages of the ascending and descending medians for all four Os, and the differences between these averages and the geometrical and arithmetical means. It will be noted that for three of the Os

(*A*, *B*, and *C*) the bisections occur close to the geometrical mean. For *B* the difference is only 4 cents. *D* finds the midpoint approximately half way between the geometrical and

TABLE I

PSYCHOPHYSICAL BISECTIONS OF THE INTERVAL 192–448 D.V.

Abbreviatons: av = average of ascending and descending orders, Dg = difference between average median and geometrical mean, Da = difference between average median and arithmetical mean

l = 192, geometrical mean = 293.3
h = 448, arithmetical mean = 320

Os	av	Dg	Da
A	296	2.7	24
B	294	0.7	26
C	299	5.7	21
D	308	14.7	12
Average	299	6	21

arithmetical means. The average for the four Os [17] falls nearer the geometrical than the arithmetical mean, but the dispersion of the separate values is greater than that for any of the intervals smaller than an octave.[18]

For Series II the limiting stimuli were 168 d.v. and 460 d.v.: an interval (1744 cents) about half way between the eleventh and the diminished twelfth. The *m* stimuli ranged between 264 and 344. The geometrical mean for the interval is 277.9, and the arithmetical mean 314. A glance at Table II

TABLE II

BISECTIONS OF THE INTERVAL 168–460 D.V.

l = 168, geometrical mean = 277.9
h = 460, arithmetical mean = 314

Os	av	Dg	Da
A	285	7.1	29
B	283	5.1	31
C	298	20.1	16
D	314	36.1	∞
Average	295	17	19

[17] The average here and in the following tables is of small statistical significance since it is compounded of values which undoubtedly represent quite different psychological trends. It may serve to indicate, however, the absence of the more unequivocal tendency which was revealed with intervals smaller than an octave.

[18] Pratt, *op. cit.*

reveals a wider divergence of the medians. For *A* and *B* the midpoint is still near the geometrical division, but for *C* the bisection comes somewhat nearer the arithmetical mean and for *D* it coincides exactly with the arithmetical mean, which brings the average value for the four Os equidistant from the two means.

The limiting stimuli for Series III were 160 d.v. and 480 d.v.; the musical interval of the just twelfth (1902 cents). The intermediate variable stimuli ranged from 268 to 332. The geometrical mean for the interval is 277.1, and the arithmetical 320. From Table III it can be seen that the results are similar to those of the preceding interval: the average value for the four Os is equidistant from the geometrical and arithmetical mean. *B* still remains at the former value—a difference of 6 cents. Both *C* and *D* are considerably nearer the arithmetical than the geometrical mean, and *A*'s value has begun to move up towards the former point and away from the latter.

TABLE III

BISECTIONS OF THE INTERVAL 160–480 D.V.

l = 160, geometrical mean = 277.1
h = 480, arithmetical mean = 320

Os	av	Dg	Da
A	293	15.9	27
B	278	0.9	42
C	308	30.9	12
D	311	33.9	9
Average	297	20	22

In Series IV the limiting stimuli were 144 d.v. and 504 d.v.: an interval (2169 cents) slightly smaller than the minor fourteenth. The variable stimuli ranged from 260 to 348. The geometrical mean for this interval is 269.4, and the arithmetical 324. The figures of Table IV show that there is here a still greater tendency for the Os to find the midpoint of the interval in the region of the arithmetical mean, with the exception of *B* who continues to bisect the distance at the geometrical mean,—a difference, in this case, of less than 4 cents. The value for *D* coincides exactly with the arithmetical mean, and *C*'s is only slightly below. *A* has continued

to move farther away from the geometrical mean so that his point of bisection is about equidistant from the extremes. The average value of the four Os is now a bit nearer the arithmetical mean.

TABLE IV

BISECTIONS OF THE INTERVAL 144–504 D.V.

l = 144, geometrical mean = 269.4
h = 504, arithmetical mean = 324

Os	av	Dg	Da
A	291	21.6	33
B	270	0.6	54
C	319	49.6	5
D	324	54.6	00
Average	301	32	23

Significant differences are apparent when the Os are considered individually.

D's values lie consistently around, and in two cases exactly at, the arithmetical mean, with the exception of the first interval, and even here his point of bisection is about as near one extreme as the other. For him equal-appearing intervals tend to be paralleled by equal vibration-differences. He was the most unmusical of the Os and had no sense of interval in the ordinary musical meaning of the phrase. If the musical judgment cut across the psychological criterion it was certainly below the level of introspective report. His own characterizations of his procedure emphasized a datum of judgment not unlike G. E. Müller's *Kohärenzgrad*.[19] If the second tone tended to be qualitatively "absorbed" by the first tone, the second distance was judged greater; if by the third, it was judged less. These qualitative absorptions were frequently supplemented by kinæsthetic cues and adjustments.

The bisections of *C* start nearer the geometrical mean for the first interval, but lie progressively nearer the arithmetical mean for the three following intervals. As the size of the interval increases the psychological divisions tend to be correlated with vibration-differences which approach equality. Observer *C*, however, is very musical and has a keen ear for the musical quality of intervals.[20] She was quite emphatic, however, in her denial of the influence of interval-quality in these experiments. In general her assurance that she was abstracting without difficulty from interval-quality is borne out by the results shown in the tables. How, *e.g.*, could a person with any musical sense at all call the midpoint of the musical interval 160–480 (a twelfth) 308 instead of 277? Here is a difference of 183 cents (nearly a whole tone) which could hardly fail to be perceived by *C* if she were watching for musical effects. Or again, in Table IV, she was off the musical midpoint by 294 cents, an interval equal to the

[19] G. E. Müller, *Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik*, 1904, 237.

[20] Both *C* and *B* were observers in an experiment on fineness of discrimination of musical intervals reported by H. Moran and C. C. Pratt, Variability of judgments on musical intervals, *J. Exper. Psychol.*, 1926, 9, 492–500.

Pythagorean minor third! Her judgments were mediated, rather, by sheer amount of difference in pitch-quality between the tones presented, supported by schematic visual imagery—a judgment comparable, perhaps, to what has been called in this connection *Helligkeitsunterschied*.[21]

The judgments of *A* are somewhat like those of *C* in that they fall, for the first interval, near the geometrical mean but move thereafter towards the arithmetical, although they do not come so close to the latter as do the judgments of *C*. The tendency, however, to move away from the geometrical and towards the arithmetical mean is clear enough. Observer *A*, like *D*, is an unmusical O with no sense of interval. His introspective characterizations were similar in many respects to those given by *C*. He referred to his datum of judgment as *degree of difference* presented by the two pitches of the auditory distance. This distance was occasionally represented by a visual vector on a vertical axis. Fairly often the first distance was judged absolutely as large or small and the judgment then corrected or verified by the appearance of the second distance.

B was the only O whose judgments remained consistently close to the geometrical mean throughout the experiments on the four intervals. *B* is a musical O with even a keener sense of interval-quality than *C*.[22] But unlike *C* she was well aware that her judgments were largely determined by musical knowledge. "In difficult cases I made my judgments on the basis of musical knowledge. If, for instance, I conceived of the lowest note as *c* and the highest note as somewhere around *b* flat (*A* always knew when an interval was not an exact musical one) of the octave above, the proper middle note, musically, would be about half a tone higher than the octave below the top note. I compared the presented middle note with the image of the proper middle note, and made judgments accordingly." This procedure was followed for difficult cases (around equality), but for simpler judgments the criterion was degree of similarity of the pitch-components. This judgment was frequently checked by "comparison of the amounts of kinæsthesis (of singing or whistling) involved in passing from reproduction of the lowest note to the middle one, and from the middle to the highest one." For an O who has had considerable practice in singing, the kinæsthetic cues of singing or whistling would undoubtedly be those built up on the basis of interval-quality. It is probably safe to assume, then, on the basis of *A*'s introspections and her consistent geometrical bisections (as contrasted with the tendency of the other Os to move up towards the arithmetical mean) that her judgments were largely determined by interval-quality rather than by sense distance.

The results for the four Os may be seen more clearly, perhaps, on Chart I. On this chart are included, in addition to the intervals of the present experiment, the three intervals smaller than an octave previously reported. The average bisections for each of these three intervals are represented by single dots since the individual items are too close to the average to be indicated. But for the intervals of the present experiment the values of each O are separately represented.

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[21] Abraham & von Hornbostel, *op. cit.*, 237.

[22] Moran & Pratt, *op. cit.*, 497.

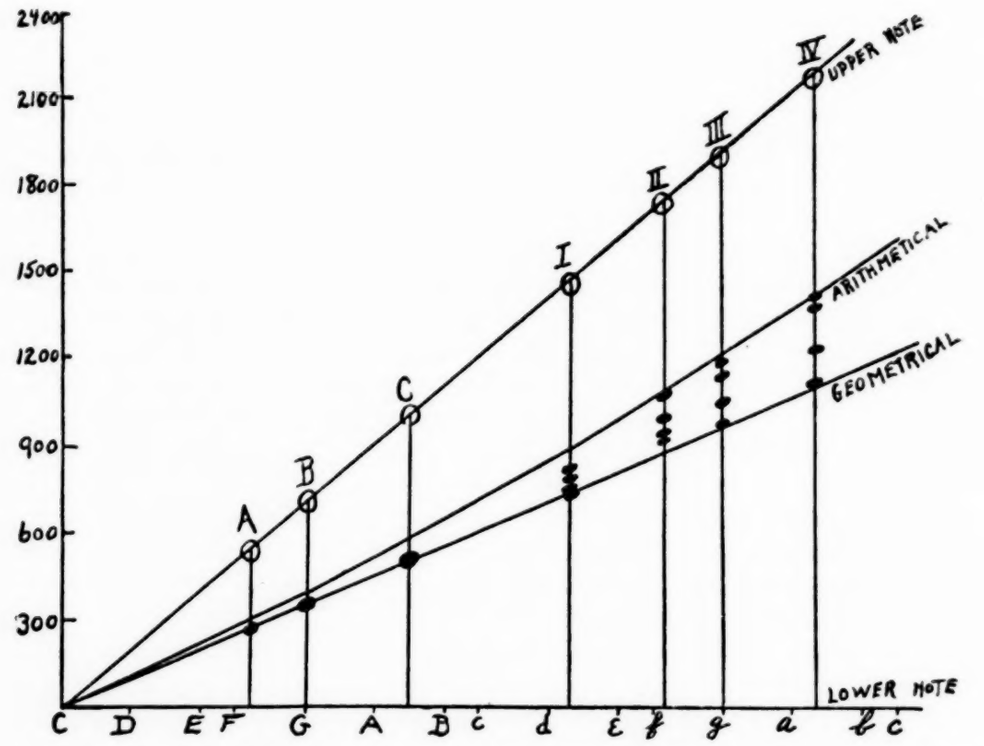

CHART I. Bisections of three tonal intervals smaller than an octave (*A*, *B*, and *C*), and four tonal intervals larger than an octave (I, II, III, and IV). Ordinates represent size of intervals in terms of cents; abscissæ, in musical notation on scale of *C*. The dots represent the observed midpoints of the intervals with respect to the geometrical and arithmetical means. (See text.)

One may see at a glance the relation of the observed midpoints to the lines of the geometrical and arithmetical means.

From these results one may conclude that for intervals smaller than an octave the midpoint lies at the geometrical mean, no matter whether the interval is perceived as an auditory distance [23] or as a musical quality. But the apparent size of intervals larger than an octave depends on the attitude of O. If O takes the auditory distance as a *musical interval* its midpoint still coincides with the geometrical division, but if he takes it as a given separation of auditory qualities the midpoint tends to move up towards the arithmetical mean.

[23] Abraham & von Hornbostel, *op. cit.*, 245 ff.

[MS. received August 9, 1927]

THE INTERPRETATION OF HANDEDNESS

BY NOEL B. CUFF[1]

Is the preferential use of one of the hands caused by a functional dominance of the right or the left eye? Parson has recently (13) suggested that it is, and he has proposed a method for detecting the ocular dominance, which he looks upon as native and inborn. The method amounts in practice to a means for determining the native difference in the use of the two hands. Parson's theory of the connection of ocular with manual functions is one issue of much study and speculation upon the question of righthandedness.

Functional preference for the right hand extends far back (how far no one knows) into human history. All known languages are said (15) to include terms for the left and right. "We have also the mute testimony of many Assyrian, Greek, and Egyptian sculptures and drawings, proving in more ways than one that for the most part the ancient peoples were righthanded" (13). Stevens (16) says: "Nothing in historic or prehistoric times is more satisfactorily established than that the people who inhabited the sheltered and sunny riversides of southern Europe . . . were emphatically and, so far as we have learned, almost unanimously righthanded." In speaking of the antiquity of the problem, Jastrow contends that "lefthandedness has always been exceptional, and righthandedness the normal" (9).

The left hand, in addition to being recognized as inferior, has had many superstitious ideas of ill-luck connected with it. This is illustrated by the Biblical narrative which informs us that Rachel, who died in giving birth to her youngest son, called him Benoni, "son of my sorrow"; but Jacob, the father, fearing that this might bode evil for the boy changed his name to Benjamin, "son of the right hand" (5). Even the present custom of wearing the engagement ring on the left hand is held by some to be a relic of primitive efforts to drive evils and temptations away from the unlucky side (15). The ill-repute of the left hand is also emphasized by Parson in his statement that some native tribes of South Africa assign to it all degrading manual acts and that "to offer food, water, or tobacco to another with the left hand is a monstrous insult" (13).

The importance of handedness is emphasized now by the extreme righthanded nature of our physical and mechanical environment (2). When we read the daily newspaper, a book, or a letter, it is necessary to begin in the upper left-hand corner and read toward the right if the symbols are to have meaning. Even in the kitchen the pans and kettles are prepared with spouts convenient for righthanded cooks. Similar examples might be indefinitely added.

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[1] A study carried out under the direction of Dr. Joseph Peterson of the Jesup Psychological Laboratory, George Peabody College for Teachers.

Other factors alleging the importance of handedness are brought out in a recent article (1) which repeats the widely advertised contention that not only does stammering occur with a frequency much greater in 'sinistrals' than in 'dextrals' but that there is also "a suspicion of relative inferiority in the organization of the central nervous system" (1) of the lefthanded. Downey and Payson raise the question as to "whether or not unidextrality, or rather degree of unidextrality, is bound up with certain mental attributes that, taken together, constitute a somewhat definite mental type" (4). Mirror writing, *i.e.*, "lefthand-writing executed by an exact reduplication of the movements of the right hand in a symmetrical way from the central point in front of the body out toward the left" (2) is another significant fact.

The many theories upon the causes of handedness may be subsumed under three classes.

(1) Habit Effects. Humphry (8), Sir Thomas Browne (3), Kellog (11) and others have held that handedness is the result of training. Advocates of this theory emphasize nursing and early education. We are told that in nursing one hand is restrained and the other left free to develop. If this is the cause of handedness, lefthanded nurses ought to secure results opposite to those secured by right-handed nurses. This does not seem to happen. Furthermore Mrs. Woolley, after conducting a careful experiment with her child, in which suggestion and training were practically eliminated said, "the tests are one more proof of the already accepted view that right-handedness must be a normal part of physiological development, not a phenomenon explicable by training" (19).

(2) Circulation. The contention by Buchanan (13) and others that handedness is determined by visceral distribution and displacement of the center of gravity in the final analysis resolves itself into a question of circulation. The argument is made that unequal distribution of the viscera causes one leg and foot to develop more than the other, which in turn gives a firmer support for corresponding hand and arm movements. If this theory be true cases of transposed viscera should be characterized by lefthandedness and lefthanded persons should have a displacement of viscera, but such is not found to be true (13, 2).

A view of the circulation theory similar to that of reversed viscera is advocated by Hyrtl (1860). He insisted that "ordinarily we find greater pressure of blood in the right subclavian artery, and, consequently, righthandedness; but that whenever the left subclavian had its origin before the right it would be found that the left side of the body was the better nourished and that lefthandedness naturally resulted (13). Since this view is so similar to that of reversed viscera the criticisms given above may be summarized here by saying that the theory is not borne out by observation and ascertained fact. Smith has also criticized the view by saying that "Wilson considers this a curious idea as it traces right-handedness to the excess of a compensating force for an assumed inferior circulation pertaining naturally to the right side" (15).

The strongest advocates of the circulation theory have insisted that handedness is determined by the blood supply of the brain. "Gratiolet (7) attributes right-handedness to the early stages of foetal development in which the anterior and middle lobes of the brain on the left are in a more advanced condition than those on the right side." Judd (10) expresses a similar view by saying: "The two sides of the brain receive their blood supply through arteries which are asymmetrical. Where the blood supply is larger to the left side of the brain, the right hand is naturally developed to a higher degree of dexterity; where the right side of the brain receives the greater blood supply, the person is naturally lefthanded." Jastrow (9), too, believes that "right-

handedness and superiority of development of the left cerebral hemisphere are the rule"; but he says "as to the explanation of the greater development of the left hemisphere, as determined by a better blood supply, or by a general advantage of the left side in nutrition, etc., little that is definite can be offered." This view is invalidated by the fact that the anterior communicating artery connects the two cerebral arteries of the brain and forms part of the Circle of Willys, with the result that the cerebral blood supply is equalized (2). The theory that handedness is connected with cerebral circulation is further invalidated by the fact that rings cut from the carotid arteries within the skull do not show the alleged difference in caliber between the right and left carotids (13).

(3). Ocular Dominance. Gould claims that in about 96 per cent. of infants the right is the better eye and "compels the right hand to work with it" (6). Stevens advanced serious objections, which are still unanswered, to this view. He says: "that the right or left hand should come to be used exclusively for all highly specialized activities as a consequence of the right or left eye being more nearly emmetropic than the other, seems to the reviewer to be untenable for several reasons." "In binocular vision," he continues, "it is impossible to distinguish the field of vision of one eye from that of the other. To all intents and purposes the two eyes function as one." Since the field of vision would be uniformly clear or disturbed there would be no motive here for the use of one rather than the other side of the body.

Stevens further points out, still referring to Gould's work, that "as a matter of neurology, as, no doubt, the author is fully aware, the macular region of each retina is connected with both hemispheres, and it is only the corresponding peripheral regions which are exclusively associated with one or the other hemisphere" (17). This neurological fact does not indicate that one side of the body is used more, due to the dominance of one eye.

Stevens' experiments led him to conclude that "By reason of a marked difference in the space sense of the two halves of the retina, those objects in the right half of the field of vision, by appearing larger, attract the visual attention which in turn leads to grasping movements with the right hand" (18). The hand thus favored develops the greater precision. A fatal objection to this theory, whose experimental support is also questionable, is the fact that among the congenitally blind one finds about the same proportion of right- and left-handedness as among the sighted (2). Still another view of ocular dominance is held by Parson. He believes that handedness is due to unilateral sighting.

In his book (13) Parson refers to the well-known experiment, concisely stated by Seashore (14), that if one holds two objects (such as a pen and a pencil) in the same line of regard, the former about 20 and the latter about 40 centimeters away from the eyes, and fixates the pen while the eyes are thus converged, one sees two pencils. The pencil, which is the distant object, is doubled homonymously, which is to say that the righthand image belongs to the right eye and the left image to the left eye. If one fixates the pencil, which is farther away from the eye than the pen, the pen is seen in

crossed or heteronymous images, the left image belonging to the right eye and the right image belonging to the left eye. He admits that, on a superficial view, the experiment just discussed seems to justify the conception of a cyclopean eye employing a median line of sight, also that the double images apparently occupy corresponding positions on either side of the median plane and that when we combine them by proper adjustments of the ocular axes the resultant single images invariably lie in the median or cyclopean visual line; but he introduces and defends 'unilateral sighting' in place of the 'double eye' or 'cyclopean eye.' He holds that, due to the 'rather strange circumstance' of their possessing true binocular vision, Hering, Helmholtz, and Le Conte all favored the cyclopean eye view (13). The evidence that they possessed true or impartial vision he deducts from their writings, and he holds that it is peculiar since impartial binocular vision is, in his view, comparatively rare. Parson's position is, of course, far from that usually taken, one that he does not seem clearly to understand and appreciate. His misunderstanding of the authorities mentioned may be clear if we quote another psychologist holding a view similar to theirs, indeed the view now commonly held by psychologists. C. S. Myers says: "Under ordinary circumstances we localize binocularly-seen objects in a direction midway between the two eyes, and in uniocular vision our localization rests usually on this basis. In other words, the two eyes, whether used separately or combined, tend to function in regard to localization as a single median 'cyclopean' eye. This tendency has to be suppressed in shooting and similar exercises involving the use of one eye" (12). It is obvious that Myers does not assume a cyclopean eye but refers to it merely by analogy, and that he assumes an equality of influence between the two eyes as the usual condition, except when sighting and demands for similar behavior arise. To prove his own point, Parson states, however, that the previous experiment is artificially produced and suggests that it be replaced as follows. "Converge the ocular axes upon a point in the median plane and about five feet distant; then, with both eyes open, interpose a

finger as nearly as possible in the median line of sight about two feet from the eyes, so as to cover the distant point of fixation" (13). In the former experiment heteronymous images of the finger were at equal distances on either side of the median plane; but in this, as Parson says, if we are right-eyed we see the image belonging to the right eye on the median line of sight while the image belonging to the left eye is at some distance to the right. This is proved, he holds, by three facts; to wit, (1) If we alternately close the right and left eyes, we find that the finger is interposed "*along the line of sight of the right eye*" (still supposing that we are right-eyed) instead of along the median line of sight; (2) If we slowly shift the gaze from point *P* to the finger we note that the double images do not seek a midway fusion-point, but that the image belonging to the right eye "*remains stationary on the right visual line*, while that belonging to the left *travels the entire intervening distance* in order to unite with its fellow image"; and (3) The image belonging to the left eye is shadowy and unsubstantial and is usually quite disregarded. The conclusion is then drawn that "unaided natural vision is bilaterally asymmetrical, and that in matters of exact orientation we are virtually one-eyed." Here it is obvious that Parson does not sufficiently consider the effect of previous habits of sighting and regards as innate what may well be acquired. He supports his conclusion by pointing out the evident fact that in sighting we must bring into strict alignment three points; the center of the macula, the point of regard, and some other intervening point or image, and that we cannot bring two or more images into the median line of sight simultaneously so that one eye only must be used for all sighting or aiming activities and therefore for many of the most important manual activities, thus causing handedness to be due to a functional limitation of vision. This does not, of course, show that dominance of right-eyedness is innate, and Parson is not satisfactorily meeting the facts that the field of vision is uniformly clear or dim regardless of the relative development of the two eyes and that the proportion of right- and left-handed persons among the congenitally blind is about the same as among the seeing.

While Parson thus supports his view he falls back on the primitive-warfare hypothesis to explain the initial cause for the almost universal prevalence of righthandedness. Advocates of the primitive-warfare hypothesis hold that the shield was carried with the left hand to protect the heart so that the right hand became the spear hand. Parson says of this theory that "while it is assuredly a most plausible one, it is well to remember that the shield was undoubtedly invented ages after the first club and javelin. Meanwhile, as we have seen, the manufacture and use of these alone, long before the shield was thought of, would certainly have induced one-handedness, and might even have developed general righthandedness, since in single combat, the only sort of warfare known in prehistoric times, righthanded warriors could most directly attack their adversaries on the heart side of the body." Obviously, Parson is inconsistent in falling back on any such theory, because it deals with the development of handedness rather than with eyedness and certainly does not explain why the right eye should control the right hand.

While the evidence is fairly clear that handedness is determined by heredity factors, we are still left only to speculation regarding its origin in phylogenetic history and the mode of its transmission is also much more in darkness than is true, for instance, of color-blindness.

Aside from the question as to whether eyedness causes handedness or whether both are effects of a common cause, it is desirable that we consider carefully the device which Parson has prepared to test handedness from eye preference and reach conclusions as to its validity and reliability.

Working on the theory that "handedness is caused by eyedness, the favored hand being on the same side of the body as the sighting eye," Parson has invented a device which he calls a *manuscope* for determining native handedness by finding out which visual line is used in sighting.

"The instrument consists of a small darkened box or camera-like chamber, one end of which fits over the eyes and upper part of the face after the manner of a stereoscope, from whence it tapers for 9 inches to a circular aperture 1⅛ inches in diameter at the farther end" (13). The subject looks through this manuscope at the center of a cardboard easel where the letter C is visible and the operator then exposes the letters R and L. Parson holds that if the letter to the right, R, is seen the subject is righthanded, if the one to the left, L, he is lefthanded. Parson gives the following results obtained from 877 public school children in Elizabeth, New Jersey (ave. age, 9.9 yrs).

Righteyed	Lefteyed	Impartial
608	257	12
69.30%	29.30%	1.37%

Of the 608 righteyed children he found only 4 lefthanded; and of the 257 lefteyed children, 225 righthanded and 32 lefthanded.

The four lefthanded cases in the 608 righteyed pupils are called by Parson 'right-eyed sinistrals,' who habitually preferred the right visual line but whose original sighting eye was the left. Two were aware of eye trouble, and the other two had (as Parson reasons but does not prove) at some time had ocular trouble or their handedness had been changed due to injury of the right arm. The handedness of the lefteyed seriously disagrees with his theory that "handedness is caused by eyedness"; but he explains this as a result of training. Children although natively lefthanded are taught to use their right hand, which does not, presumably, change eyedness.

We have undertaken to give the Parson test to a considerable number of children under conditions as well controlled as practical school conditions permit. The directions given by Parson were closely followed except that precautions as to light conditions and other possible disturbing factors were observed more closely than a mere adherence to these directions would warrant.

The easel was placed in a strong, even illumination, though not in sunlight. This condition was kept constant by using artificial light at M. H. School. In the other two schools the subject was seated with his back toward a window so that the letters on the easel were clearly seen. The tests were given during April and May, 1926, in three schools of Davidson County, Tennessee. All of the pupils in any grade were tested individually in a manner insuring that no one was influenced by the responses given by another, with the exception of grade 6 of C. B. School. The pupils in that grade were all in the room while being tested. Although Parson does not suggest that the test be given more than one time, except when the result does not agree with the history of the case, our tests were given five times in succession at one sitting to each subject. The experimenter gave the test to a number of preliminary cases in order to become thoroughly familiar with the difficulties in its administration and the cautions to be observed, and he rigidly adhered to Parson's instructions in securing the results which follow.

TABLE I

MANUSCOPIC TESTS OF 146 SCHOOL CHILDREN

School	Grade	Ave. Age Yr	Ave. Age Mo	No.	Righteyed M	Righteyed F	Righteyed Ttl	Lefteyed M	Lefteyed F	Lefteyed Ttl	Impartial-eyed M	Impartial-eyed F	Impartial-eyed Ttl
M. H.	3	9	7	19	6	6	12	3	4	7	0	0	0
M. H.	4	11	8	18	5	4	9	8	1	9	0	0	0
M. H.	5	11	11	24	10	7	17	3	1	4	0	3	3
M. H.	6	12	9	23	8	10	18	2	0	2	0	3	3
C. B.	6	13	4	10	4	1	5	1	2	3	0	2	2
M. H.	7	13	2	10	2	2	4	2	1	3	1	2	3
C. B.	7	13	5	9	4	4	8	0	1	1	0	0	0
M. H.	8	14	7	9	1	1	2	1	4	5	1	1	2
C. B.	8	14	4	10	3	3	6	4	0	4	0	0	0
U. H. S.	9	14	7	12	4	5	9	1	2	3	0	0	0
U. H. S.	10	14	9	2	0	1	1	1	0	1	0	0	0
		12	11	146	47	44	91	26	16	42	2	11	13
				100%			62.3%			28.8%			8.9%

TABLE II

HANDEDNESS OF THE RIGHTEYED, LEFTEYED, AND IMPARTIAL-EYED PUPILS

School	Grade	Righteyed (91)						Lefteyed (42)						Impartial-eyed (13)					
		Righthanded			Lefthanded			Lefthanded			Righthanded			Lefthanded			Righthanded		
		M	F	Ttl	M	F	Ttl	M	F	Ttl	M	F	Ttl	M	F	Ttl	M	F	Ttl
M. H.	3	5	6	11	1[a]	0	1	0	0	0	3[h]	4[i]	7						
M. H.	4	5	3[b]	8	0	1[c]	1	0	0	0	8[j]	1	9						
M. H.	5	10	7	17	0	0	0	0	1	1	3	0	3	0	1[l]	1	0	2[m]	2
M. H.	6	7	10	17	1[d]	0	1	0	0	0	2	0	2	0	0	0	0	3[n]	3
C. B.	6	4	1	5	0	0	0	0	0	0	1	2	3	0	0	0	0	2[o]	2
M. H.	7	1	2	3	1[e]	0	1	0	0	0	2	1	3	0	0	0	1[p]	2[q]	3
C. B.	7	3	4	7	1[f]	0	1	0	0	0	0	1	1						
M. H.	8	1	1	2	0	0	0	0	0	0	1	4	5	0	0	0	1[r]	1[s]	2
C. B.	8	3	3	6	0	0	0	0	0	0	4[k]	0	4						
U. H. S.	9	4	4	8	0	1[g]	1	0	0	0	1	2	3						
U. H. S.	10	0	1	1	0	0	0	0	0	0	1	0	1						
		43	42	85	4	2	6	0	1	1	26	15	41	0	1	1	2	10	12
				93.4%			6.6%			2.4%			97.6%			7.7%			92.3%

a. Lefthanded in everything; mother knew of no ocular or manual trouble. *b*. One case used the left hand to bat and throw a ball but used the righthand for everything else. *c*. Lefthanded for everything except writing. *d*. Saw neither letter before moving farther from the test than the specified two feet; then he consistently saw the letter R. *e*. Frequent sties on the left eye and left arm broken twice. Lenses worn; right 1.2 + Em = 1.2 + As., left, 1 + Em = 1 + As. *f*. Had always been lefthanded in everything. *g*. Always lefthanded and positive that nothing had been wrong with either eye. *h*. A careful study of one case revealed that he had always been righthanded. *i*. All 4 testified that they had always been righthanded and had never had any eye trouble. *j*. Two cases admitted having been lefthanded at one time. *k*. Formerly lefthanded. *l*. Saw R2 (two times) and L3 (three times) in succession; always righthanded. *m*. A saw R2, L 3; B saw L first and then alternated. *n*. A saw R4, L1; B saw L3, R2; C saw R2, L3; and each claimed always to have been righthanded. *o*. Both saw R3, L2. *p*. Saw both letters before moving nearer; then saw L3, R2. *q*. A saw L3, R2. Lenses, right, .8 + Hy = .9 + As, left, .8 + Hy = .8. Righthanded in everything. B saw both letters; on moving nearer she saw R3, L2. *r*. Trouble with right eye; lefthanded when a baby; now consistently righthanded. *s*. Saw L1, R4.

Since Parson discusses handedness in terms of eyedness, we thought it might be suggestive to give tests of visual acuity and compare the results with those secured by using the manuscope.

We gave these tests to grades 7 and 8 in M. H. School. They were given according to the directions for Test 14 in Whipple's *Manual of Mental and Physical Tests; Part I Simpler Processes.* In all, 19 subjects were used. Ten of the subjects, according to the tests, had equal vision of the right eye and left eye. Six of them had better vision with the right eye and three better with the left. The lack of agreement between the acuity tests and the manuscopic tests may be seen in the following Table III.

TABLE III

VISUAL ACUITY TEST			MANUSCOPIC TEST								
Right Eye = Left Eye			Righteyed			Lefteyed			Impartial-eyed		
M	F	Ttl	M	F	Ttl	M	F	Ttl	M	F	Ttl
5	5	10	1	2	3	2	2	4	2	1	3
R. E. > L. E.											
3	3	6	2	0	2	1	1	2	0	2	2
R. E. < L. E.											
0	3	3	0	1	1	0	2	2	0	0	0

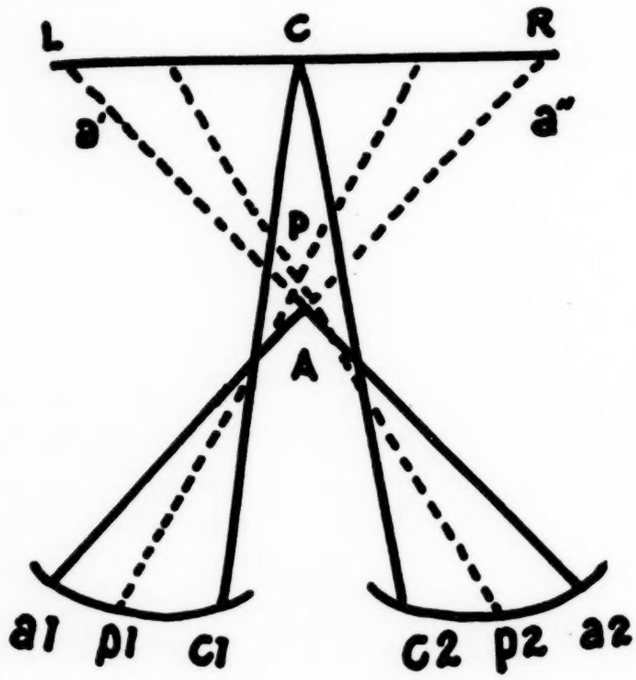
The percentages of righteyed (62.3) and lefteyed (28.8) found in our study agree only roughly with the results obtained by Parson; our impartial was 8.9 per cent., whereas he found only 1.4 per cent. Out of 608 righteyed pupils, he found only 4 (.7 per cent.) lefthanded while we found 6 (6.6 per cent.) in 91 cases of righteyed pupils. The present results show that 2.4 per cent. of the lefteyed are lefthanded while his show 12.4 per cent. In speaking of impartial vision, he argues that the sighting is lateral but fluctuates from side to side (13). Our results show over 7 per cent. more impartial cases than do Parson's.

A Criticism of the Manuscope

1. The directions for giving the test are inadequate and not conveniently prepared. Part are given on page 79 and part on page 82 of the author's book. It is suggested by Parson that the easel be about two feet in front of the subject, which will not, according to our experience, secure consistent results; *e.g.*, at the specified distance some subjects (see

notes *p* and *q*) saw both letters and one (note *d*) did not see any. The subjects who saw both letters on moving nearer saw only one letter at each trial and the subject who did not see any letter had to move farther away.

The accompanying diagram more fully explains these results. *C* is the fixation point on the easel, rays from which fall on the corresponding points c_1, c_2, of the retinæ. *A*, the aperture at the small end of the manuscope, lies nearer than the fixation point so that *a'* and *a''* are heteronymous images of *A* due to disparation of the retinal points a_1 and a_2. This diagram is drawn to represent the individual who sees both *L* and *R* when the distance is 2 feet from *A* to *C*. If the distance from *A* to *C* be shortened so that it equals the distance from *P* to *C* it is obvious that the individual will not see



either *L* or *R*. Hence the distance between *A* and *C* has to be gradually decreased until the subject cannot fixate on *C* and, at the same time, see both *L* and *R*. Then the subject is forced to point the manuscope slightly toward *R* or *L* in order to see either letter. Since the manuscope is pointed toward the right or left, the experimenter can tell which letter the subject sees even before it is pronounced. It is evident from the diagram that the person who did not see either *L* or *R* needed to move farther away from the easel. The distance used with the special cases varied from 18 to 60 inches. (We do not mean to say that all of this distance is due to difference in distance between the eyes. Subjects were allowed to hold the manuscope as they chose except that they were instructed to grasp it with both hands. Hence, some pointed it toward the center of the easel while others pointed it slightly toward the right or left.) Therefore, the specified distance of 2 feet is not one that can be used with all subjects and yet a definite distance is important because the subject is reacting to the test while the experimenter is trying to determine the proper distance.

2. No provision is made for adequate control. The persons who see *L* sometimes and *R* sometimes are termed impartials (Parson) because they are theoretically ambidextrous. Our results indicate that the impartial-eyed are no more nearly ambidextrous than are the righteyed. According to Table II, 93.4 per cent. of the righteyed are righthanded and 92.3 per cent. of the impartial-eyed are righthanded with not a single ambidextrous case. It seems to me from careful observation that two other more plausible explanations account for the impartial-eyed cases: (*a*) Some subjects are careless about following directions in regard to fixating on the center of the circle and curious to see everything and therefore allow their eyes to fluctuate. One subject (see note *m*) pointed the manuscope toward *L*, then toward *R*, and continued to shift back and forth. (*b*) Not having a practice test, even though every possible precaution be taken to make all subjects understand just what they are to do, some may not understand the procedure. All of the subjects listed as impartial-eyed with one exception saw either *L* or *R* consistently before shifting to the other letter which they saw thereafter. This seems to indicate that the letter first seen was merely a result of chance and that it continued to be seen for a number of trials before the subject shifted to the other letter.

3. The statistical data given in regard to norms are inadequate. If the test had been given to the same children at a later time the reliability of the results could have been determined. Our tests were given five times in immediate succession and, with the exception of the 13 impartial-eyed, the subjects saw the same letter each time. Our results, however, show (as can be seen from the summary following the tables) that the validity and reliability of the test are not beyond question.

4. The disposition that is made by Parson of the four lefthanded pupils found in 608 righteyed pupils is unsatisfactory. It is said that their sighting eye had changed due to ocular troubles or their handedness had changed due to trouble with the hand. This is only a theoretical explanation.

If Parson's theory of handedness were true, the righteyed would be righthanded; but 6.6 per cent. of them are, according to our results, lefthanded. This cannot be explained as a result of training, and our subjects insisted that it was not due to trouble with either the left eye or the right hand.

5. Our discussion indicates that even if the gross results are fairly reliable, the results for individual cases are questionable. With 6.6 per cent. of the righteyed lefthanded, it is safe to assume that some of the lefteyed who are righthanded are to be explained in the same manner. We have already objected to Parson's disposal of his impartial cases (8.9 per cent. in our study). Hence all of these exceptions to his theory of handedness as a result of eyedness amount to approximately 20 per cent. of the number tested. One serious criticism that Parson has made of other tests is that they are not reliable for individual diagnosis. From the preceding statements it seems that we need more results to persuade us that the same criticism does not also apply to his manuscopic test.

6. After using the manuscope and after studying other methods which have been advocated for testing handedness, we think that the manuscope represents a certain advance in technique.

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NEGATIVE ACCELERATION WITH MATERIAL OF VARYING DIFFICULTY

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The hypothesis that the phenomenon of negative acceleration in memory curves is due, in part, to the unequal difficulty of the various items, is based on the three following assumptions; that the various items in the list will necessarily vary in difficulty, that the subject will tend to learn the easy items first reserving the harder items for the later trials, and that the number of items learned in any one trial will be a function of their difficulty.

On the basis of the above assumptions, it is obvious that more items will be learned in the earlier than in the later trials if no other influences interfere, and hence the curve will tend to exhibit negative acceleration.

The following experiment was designed to test this hypothesis by constructing lists which were known to consist of equal numbers of easy and hard items, submitting them to a group of subjects for memorization, and noting in which trials the various items were learned.

The items were pairs of one-syllable words. We first constructed fourteen lists of sixteen pairs each in which there was no obvious relation between the words of a pair. These, presumably, were the difficult lists. Likewise, we constructed fourteen easy lists of sixteen pairs each in which the associated words were related. All pairs of each list exhibited the same type of relationship, but the kind of relation varied from list to list. Fourteen relationships were thus employed: (1) object-use, (2) noun-attribute, (3) particular-general, (4) synonyms, (5) rhymes, (6) things experienced together, (7) logical relations, (8) different grammatical forms, (9) old, well-known associations, (10) part-whole, (11) cause-effect, (12) spatial relations, (13) similarity in spelling and (14) opposites.

To test their difference in difficulty, the lists were presented to three subjects in the order easy-hard to avoid practice effects. The results are given in Table I. X Y, and Z refer to the three subjects, E and H distinguish the easy and hard lists, and the figures below show the number of trials required to learn the various lists. At the bottom of the table are the totals and in the last column are the totals for all subjects. A total of 100 trials was required to learn the easy lists and 374 trials to learn the hard

lists. In no case did a subject learn a hard list quicker than an easy list. The lists are therefore unequal in difficulty.

Eight of the pairs from an easy list and eight of the pairs from a hard list were then combined to form a new list of sixteen pairs of unequal difficulty. The words chosen were those missed least often among the easy pairs and those missed most often among the hard pairs. In this way fourteen lists of unequal difficulty were constructed. In half the lists the easy items were placed in the even positions, and in the other half in the odd positions in order to eliminate the influence of the position factor.

These lists of mixed materials were presented to six adult subjects. The words were presented on cards, one pair of associates to a card. For recall another pack of cards was used with the stimulus word only on them. After each presentation the subjects responded to the stimulus word on the recall cards. This procedure continued until the responses were correct for all sixteen words of a list. Both in presentation and in recall three seconds were allowed for each card. Between presentation and recall and between recall and the next presentation approximately ten seconds of time elapsed. Two minutes were thus required for one presentation and one recall.

TABLE I

	Subject							
	X		Y		Z		Combined (X, Y, Z)	
List No.	E	H	E	H	E	H	E	H
1	1	4	2	5	1	6	4	15
2	1	6	2	9	2	28	5	43
3	1	5	1	9	2	15	4	29
4	2	8	3	9	4	14	9	31
5	3	8	3	5	6	31	12	44
6	2	5	2	8	3	14	7	27
7	2	4	2	4	4	17	8	25
8	3	5	2	6	9	14	14	25
9	1	3	1	8	2	13	4	24
10	2	4	2	6	2	14	6	24
11	2	4	2	8	6	19	10	31
12	1	3	2	4	3	11	6	18
13	2	3	2	5	4	10	8	18
14	1	3	1	5	1	12	3	20
Total	24	65	27	91	49	218	100	374

A record was kept of the particular items that were memorized in each presentation and we then tabulated for each subject and each list the number of easy and hard stems that were learned for each successive trial. All records that involved the same number of trials were now grouped together, and the average numbers of easy and hard items learned in the successive trials were computed. These data are presented in Tables II to XI. For example, there are twenty-one cases in which the lists required five trials for memorization, and these were distributed among five of the six subjects. The average numbers of easy and hard items learned in each of these five trials are given in Table IV.

TABLE II

THREE TRIALS

Average Data for Three Records of Three Subjects

Trial	1	2	3
Easy	6.7	1.3	0
Hard	3.3	3.0	1.7

TABLE III

FOUR TRIALS

Average Data for Twenty Records of Five Subjects

Trial	1	2	3	4
Easy	6.7	1.0	.2	.1
Hard	2.0	2.4	1.7	1.9

TABLE IV

FIVE TRIALS

Average Data for Twenty-One Records of Five Subjects

Trial	1	2	3	4	5
Easy	6.5	.6	.6	.2	.1
Hard	1.4	2.4	1.8	1.0	1.4

TABLE V

SIX TRIALS

Average Data for Eleven Records of Five Subjects

Trial	1	2	3	4	5	6
Easy	5.7	1.3	.4	.2	.2	.2
Hard	.6	1.3	1.6	1.2	1.3	2.0

TABLE VI

SEVEN TRIALS

Average Data for Seven Records of Six Subjects

Trial	1	2	3	4	5	6	7
Easy	6.0	1.2	.6	.1	.1	0	0
Hard	1.0	.9	1.9	1.2	1.0	.6	1.4

TABLE VII

EIGHT TRIALS

Average Data for Eight Records of Six Subjects

Trial	1	2	3	4	5	6	7	8
Easy	6.8	0	.5	.4	.2	0	0	.1
Hard	.5	1.2	.6	.9	2.1	.5	.4	1.8

TABLE VIII

NINE TRIALS

Average Data for Eight Records of Four Subjects

Trial	1	2	3	4	5	6	7	8	9
Easy	4.6	2.3	.3	.5	0	.1	.1	0	.1
Hard	.1	.6	1.4	1.4	.8	.9	1.2	.4	1.2

TABLE IX

TEN TRIALS

Average Data for Two Records of Two Subjects

Trial	1	2	3	4	5	6	7	8	9	10
Easy	5.0	2.0	.5	0	.5	−.5	0	.5	0	0
Hard	0	1.5	.5	.5	1.0	1.5	0	1.5	.5	1.0

TABLE X

ELEVEN TRIALS

Average Data for Three Records of Two Subjects

Trial	1	2	3	4	5	6	7	8	9	10	11
Easy	5.3	1.0	.7	.3	0	0	.3	−.3	0	0	.7
Hard	0	1.7	−.3	0	1.0	1.0	−.7	2.3	.3	1.0	1.7

TABLE XI

THIRTEEN TRIALS

Data for One Record of One Subject

Trial	1	2	3	4	5	6	7	8	9	10	11	12	13
Easy	8	0	0	0	0	0	0	0	0	0	0	0	0
Hard	1	0	1	2	0	0	−1	2	−1	1	0	0	3

It is at once apparent from an inspection of these tables that the great majority of the easy items are learned in the initial trial or the early trials, while the hard items tend to be distributed rather equally.

Of the easy items 77 per cent. were memorized in the first trial, 13 per cent. in the second, with the remaining 10 per cent. being well distributed among the remainder of the trials; while, on the other hand, only 12 per cent. of the hard items were learned in the first trial.

More easy than hard items were memorized in the first trial in all ten comparisons, while more hard than easy items were learned in 50 of the remaining 66 cases.

Hard items were learned in 65 of the 76 total trials, while the easy items were distributed in but 46 of the 76 trials.

A novel and interesting fact is the phenomenon of the "final spurt" exhibited by nine of the ten tables. In these cases more items were memorized in the final trial than in the preceding trial. The phenomenon is exhibited by the hard items in nine cases, by the easy items in three cases, and by the combined scores in nine of the ten cases. So far as we are aware, this phenomenon of a "final spurt" has never been previously noted in the study of memory curves. It is possible that this "spurt" is a function of the character of our material. It is obvious, however, that the phenomenon is more likely to appear with our method of grouping and averaging the data; for such a phenomenon, if present, may be obscured by the usual methods of combining the scores.

On the basis of these results we may conclude

(1) that the factor of unequal difficulty of the items of a list may account, in part, for negative acceleration in memory curves, and

(2) that memory curves may exhibit the phenomenon of the "final spurt."

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TECHNIQUES FOR MEASURING SERIAL ACTION

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Critique of Apparatus and Principles

Motor tests of serial action seem to be more closely related to actual conditions of human performance than are the classical measurements of single reaction. The gradual recognition of this fact is shown in laboratory analyses of motor coördination, in clinical diagnoses of impaired coördination under the influence of drugs, and in the selection of persons for training in highly skilled motor performances. Dewey's early contention[2] against the arbitrary basis of classification of behavior by reflex arcs finds experimental confirmation in the unsuccessful attempts of laboratory and industrial psychologists to analyze skill in complex activities into speed of simple reaction time, steadiness, etc. In studying the effects of alcohol upon simple reactions Dodge and Benedict (13*a*) came to the conclusion that the higher centers were less impaired than were the lower centers. They attributed the maintenance of efficiency in the higher centers to what they called 'autogenic reinforcement.' Subjects reported that they could pull themselves together for short periods. It is, of course, much easier to do this under conditions of intermittent demand than in the more even and continuous process of serial action, as has been shown by the clinical studies of Miles (13), Dunlap and others (17, 18, 18*a*) upon the impairment of continuous performance under the influence of various abnormal conditions.

Two main types of motor tests for serial action may be distinguished: (1) pursuit tests employing continuous adjustive reactions to some sort of target moving in a regular

[1] The writer is indebted to the National Research Council and to Dr. W. R. Miles, under whose direction the work on motor skills is being done.

[2] Dewey, J., The reflex arc concept in psychology, *Psychol. Rev.*, 1896, 3, 357–370,

or variable path, and (2) discrimination or tachistoscopic tests in which there is a chain of unit reactions to a limited number of types, usually four, of discrete stimuli. A new and simplified apparatus for serial discrimination is described in the latter part of this paper. In both types of measurement there are alternative procedures for controlling rate or time and measuring accomplishments, or for defining the series and measuring time. Errors may be dealt with in either case.

To facilitate the comparison and selection of apparatus for use in future work the following table has been drawn up classifying each of the principal devices known to the writer, according to the date of publication, type of activity called for, kind of stimulus presented, length of series before repeating, method of recording and applications so far reported.

The various investigators listed have contributed the following criteria as desirable points in the measurement of serial action.

1. Sufficient length for statistical reliability and to avoid easy memorizing of the series.
2. Series sufficiently novel to be relatively uninfluenced by previous practice in other activities.
3. Mechanical control or elimination of as many variables as possible.
4. Ease of recording, whether by time, counters for right and wrong actions, or graphic analysis.
5. Mechanical reliability, usually gained through simplicity.
6. Rapid action of mechanism in order to avoid obscuring the reaction speed of the observer himself in the total time; minimizing of 'fade in' and 'out' of stimuli if on tachistoscopic apparatus.
7. If for vocational work, the performance should be simple enough to be easily understood and used in practical testing, and preferably inexpensive, to allow the purchase of a battery of various instruments.
8. For most cases where predictive value in actual work is desired for the test, it is better to use tests of supraliminal proficiency rather than tests of liminal discrimination.
9. Minimal distraction by the moving parts of apparatus; usually secured by use of a visual or auditory shield, or by placing apparatus in another room.
10. Stimuli constant in intensity.
11. Action to cause as little fatigue as possible unless it is desired to measure fatigue.
12. In tests of 'physiological condition' activity must be continuous without chance for rests, and should show test-results immediately to keep within limits of safety for the observer.
13. Measurement should be "time consumed in one complete and 'errorless' act; that is, any errors which may have occurred must have been corrected before the act is complete" (Max Meyer).

SUMMARY OF CONTINUOUS REACTION APPARATUS[3]

Apparatus; Pursuit (P) or Discrimination (D)	Presentation of stimuli	Variable measured; length of series before repeating; length used	Recording methods and devices	Application reported
1. Seashore psychergograph (D)	auditory or visual series, electrically controlled	liminal errors or supraliminal reaction times; series variable in length	ink pens on paper tape, electrically driven	attentional periodicities
2. McDougall dotting apparatus (P & D)	dotted tape, weight driven	errors in thrusting stylus at dots; 1200 dots in 200 seconds	stylus marks on stimulus tape record	fatigue (McDougall, Rivers, Smith); intelligence of boys (Burt)
3. Coover typewriting mechanism (D)	letters exposed singly at opening on carriage	time and errors; approx. 80 per line	kymograph	analysis of serial action
4. Bogardus fatigue apparatus; modified (P)	revolving arm to be fed with square blocks and recovered once per sec	errors weighted; 1 rev. per sec, 15 min	observation only	decreased motor coordination in fast work; motor skills (Perrin)
5. Münsterberg motorman apparatus (D)	visual stimuli on cards exposed serially by observer	time and errors on danger signals; 26 signals per card; 12 cards	stop watch	selection of street railway motormen
6. McComas app. for recording continuous discrimination (D)	four colored electric lamps flash on ground glass; electric remote control	time and errors; 60 flashes	kymograph and special marker for right and wrong acts	analysis of discrimination; attention
7. Miles pursuit pendulum (P)	pendulum of one second period emits stream of water to be caught in tube-cups	amount of water caught, 50 cc possible; one double swing of pendulum; 25 or more tubes	gauge for tubes and graduate cylinder for total	criterion of neuro-muscular condition in metabolic sleep loss, alcohol tests, etc.
8. Snoddy stabilimeter; modified (P)	mirror drawing in star-shaped notched path; touching sides records error on counter	time plus errors equals a constant; also variability; one circuit of path; one to sixty times	stop watch and electric counters; many graphic and photographic techniques useful elsewhere	motor coordination and age; spaced learning; stability in insanity

[3] The numbers at the left in the table conform to the numbering in the bibliography at the end of this article.

Apparatus; Pursuit (P) or Discrimination (D)	Presentation of stimuli	Variable measured; length of series before repeating; length used	Recording methods and devices	Application reported
9. Miles pursuitmeter (P)	wattmeter indicates changes in current produced by inharmonic motion of solenoid	error in compensating current change by rheostat lever; 8 min cycle	electrical counters, graphic recording wattmeter; standard instruments	same as for Miles pendulum (no. 7)
10. Hansen serial action (D)	typewriter with commutator for spaced electric lamps or sounders	time, errors, variability; 65 per line	stopwatch and typewritten record	selection in telegraphy, typing, music, etc., speed vs. variability
11. Koerth pursuit rotor (P)	phonograph turntable revolves embedded target at 60 r.p.m., to be followed with flexible pointer	per cent. time pointer is on target for 20 rev	Veeder ratchet counter actuated by magnet	individual differences in motor coordination (Koerth, Seashore, R. H., Travis)
12. Wishart pursuit app. (P)	electrically driven eccentric disc moves mercury cup back and forth in straight line under pointer to be kept on it.	per cent. time pointer is on target; one revolution; indefinitely	magnetically operated cut off of water flow into graduate cylinder	not stated
13. Miles typewriting apparatus (D)	ordinary typing from memory and varied copy	speed, errors, legibility; varied lengths	typewriter and record on kymograph with Dodge duplex marker	criterion of condition in alcohol study
14. Patten coordination app. (mod. P)	two worm gear cranks placed at right angles to move brass pointers and pencil in hexagonal path	time or distance traveled in completing one circuit	stop watch; watch gears for measuring distance	prediction of success in engine work of students
15. Rexroad multiple choice app. (D)	motor driven gears with electric light contacts flash 5 different colors on ground glass	time and errors; 23 reactions; changeable	Renshaw-Weiss polygraph (ink recorder)	discrimination response; shock; cond. response to colors, names

Apparatus; Pursuit (P) or Discrimination (D)	Presentation of stimuli	Variable measured; length of series before repeating; length used	Recording methods and devices	Application reported
16. Renshaw-Weiss pursuit-meter (P)	gears move target in irregular path; observer keeps pointer on target, partially controls speed	per cent. time pointer kept on target; 1386 rev., long periods	Renshaw-Weiss polygraph	postural stability during work, etc. (not yet completely described)
17. U. S. Army (Dunlap et al) "LVN" (light voltmeter, noise) (complex) (D)	worm geared contact wheels light lamps, change current in meter and motor; observer corrects circuits	reaction time and errors; 343, 824, 705 rev., long periods	none, inspectional clinical diagnosis for immediate knowledge of condition	condition criterion in aviation; oxygen loss, also tobacco (Fisher)
18. Dunlap "Felix" app. (D)	worm gear contact wheels for four lamps, to be extinguished	same as above	special ink kymograph	condition criterion
19. Seashore motor rhythm app. (D)	rhythm patterns of telephone receiver clicks produced on phonograph circuit; observer taps in time	standard deviation of reproduced pattern; four note series, 50 times	magnetically operated pen on circular disc on same phonograph	analysis of rhythmic behavior relation to musical proficiency
20. Schuster (Farmer and Chambers) pursuit meter (P)	phonograph-driven eccentric arms drive pendulum irregularly, to be followed with another pendulum	integrated error score; four different patterns, short	mechanical ratchet and pawl counter	motor skill and industrial accidents
20*a*. Schuster (Farmer and Chambers) dotting machine (modification of no. 2)	spiral disc presents holes in paper over metal disc, to be dotted with stylus, at increasing speed	no. correct thrusts	electrical Veeder counter	same as for 20
21. Seashore serial action apparatus or discrimeter (D)	weight driven escapement disc with series of color or symbol stimuli on cardboard, as fast as reacted to by Obs.	total time; 100 signals, changeable; one series or longer	stopwatch; modified Weiss-Renshaw polygraph used for graphic analysis	analysis of motor skills

This summary covers twenty-two techniques, of which eleven are tests of discrimination involving series of discrete reactions to unit stimuli, six are pursuit-tests involving continuous adjustive reaction to a moving target, and five are intermediate in the type of activity involved.[4] While the literature reaches back to 1902, the large proportion of the work which has developed within the last few years is evidence of the interest now being focused on serial action.[5]

There is a general trend toward the simplification of devices in order to measure but one variable at a time. Graphic recording of results has been made simpler by the introduction of ink tape recorders in place of smoked drums; but, except for very fine analyses, there is a further tendency to do away with graphic recording in favor of automatic counting or timing devices.

Among the applications of these techniques of serial action, the measurement of physiological condition has perhaps made the greatest progress, as in the work of Miles and Dunlap. The study of interrelations of various measures of serial action, among themselves and in relation to actual working conditions, seems to be a promising attack upon the analysis of motor skills. Among the important points yet to be evaluated are the interrelations of fine motor coördinations, the persistence of individual differences through training, and the relative importance of motor coördination versus knowledge of techniques in various motor activities. These criticisms and suggestions apply with equal force to laboratory, industrial, and personnel psychology.

[4] It is obviously impossible to include all the work on typewriting; but since the apparatus is about the same for all, a few techniques are given as representative. The writer regrets that Max Meyer's apparatus for tests of special ability (12*a*) have not yet been described in sufficient detail for classification in the table.

[5] Our table makes no reference to card-sorting or similar devices which originated still earlier, but limits itself to mechanical devices, which usually simplify procedures by eliminating or regulating a number of variables, such as mode of stimulus presentation, and kind and scope of movements.

The Serial Discrimeter

The tachistoscope shown in the figure was designed in the Psychological Laboratory of the State University of Iowa [6] to measure a relatively simple type of continuous discrimination reaction to visual stimuli of four different kinds. In previous work Coover (3) and Hansen (10), using typewriters, selected four as the optimal number of stimuli. This number has the practical advantage of permitting use of a single hand or the fingers from both hands. Since a typewriter is unnecessarily expensive, fairly slow of action, and usable only in part, we have devised a mechanical escapement which presents a circular series of signals one at a

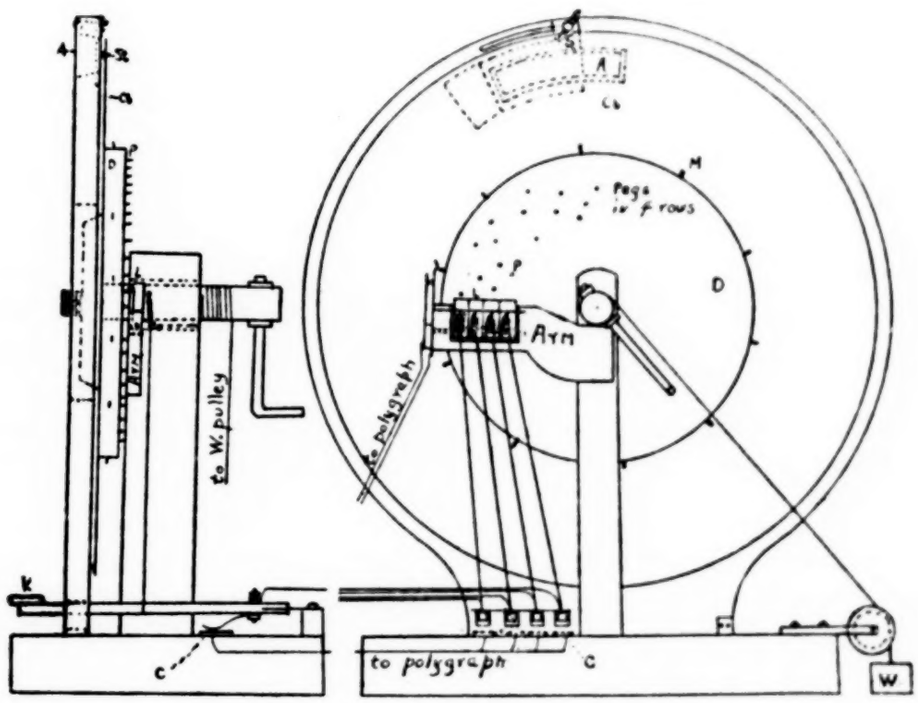

Diagram of simplified apparatus for serial action; side and rear views (No. 21 in Table)

time before an aperture. This greatly simplifies the apparatus as to number of parts, thus making it less expensive and easier to keep in repair, speeds up the key-action by making it an escapement instead of a pushing stroke, and presents stimuli without the use of electrical lamps.

[6] One of the earliest means of testing serial action was Seashore's Psychergograph (1) which has served as a starting point for a number of other pieces of apparatus, including the one here described.

A simple weight-driven escapement is released, one point at a time, by pressing the appropriate one of four keys, the object being to make 100 discrimination reactions in the shortest possible length of time. The disc *D* is under constant tension of the weight *W*. Pressing the key *K* draws back the escapement lever *L* releasing the steel peg *P* and allowing the disc *D* to rotate 3.6 degrees in a clockwise direction until stopped by the next peg, thus automatically presenting a new stimulus on the cardboard disc *Cb* at the aperture *A*.

Since each lever moves separately and controls a single row of pegs, the right key must be pressed each time before the stimulus changes. The keys may be arranged so that only one key can be pressed at a time. On the present machine one hundred signals make up a series, but since they are placed on a circular disc they can be repeated as many as ten times without rewinding, and with several pulleys can run almost indefinitely. New series can be substituted on the machine quite easily. The weight-driven feature of the apparatus has the advantage of simplicity and low cost.

Each one of the features of the test may be varied independently by the use of changeable parts. Numbers and colors have been used as signals and the fingering reversed, thus demonstrating four out of many possibilities. One, two, or three signals may be made visible by the use of a sliding shield at the aperture. The designation of the keys may be arranged in any fashion desired. The series of movements involved in one trial of one hundred reactions may be kept the same, varying the other features, or different starting points may be rotated from one trial to the next.

Ordinarily the time for a given number of reactions is measured with a stop watch. If a qualitative analysis of the serial action is desired, the four keys are arranged to make electrical contacts on the down stroke, closing a circuit through a series of magnetically operated pens, as for instance in the polygraph devised by Weiss and Renshaw at Ohio State University. This polygraph can also record simultaneously the position of the reaction in the series, by having a pen work on every tenth escapement, thus enabling easy detection of mistakes. Concurrent variables, such as posture changes, may likewise be recorded for correlation.

Possible criticisms of the apparatus are: (1) It is not noiseless; the only noise, however, is the click at the stopping of the stub pegs in the disc by the escapement lever, and since the rate of this clicking is that of the observer's action, it does not interfere. (2) Possibility of sound localization from the levers. This would call for a discrimination of 1° difference for adjacent keys, or 3° for the end keys.

[7] The present model, made in the University of Iowa Laboratories by Mr. J. B. Dempster, cost approximately $60.00.

Measurements of sound perimetry show that 1° is too fine for consistent judgment and if the observer guessed at the larger differences he should make more errors than are empirically observed. (3) Possible vibration of stimulus upon coming to a sudden stop. This is not detectible.

The principal advantages of this device, some of which are shared by other apparatus, are: (1) simplicity of design resulting in low cost and ease of upkeep (2) use of fast escapement mechanism rather than usual push-key thus making total time for a series more a measure of individual muscular speed than speed of the mechanism, (3) necessity of completing errorless series, by correcting mistakes immediately, and measuring a single variable time, (4) stimuli presented by the reacting mechanism without use of electric devices, (5) easy variation or substitution of series, and (6) action narrowed down chiefly to wrist and finger movements, simplifying analysis.

Summary

1. The writer presents a tabular summary of the tests of serial action described in the literature and a collected list of desirable features of such tests to facilitate the construction of new tests or the selection of tests for future work on experimental analyses of motor skills, physiological condition or vocational selection.

2. A new apparatus for serial discrimination is described, figured, and valued according to the above criteria. This apparatus is simple, non-electrical and adaptable to a wide variety of conditions.

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A Few Odor Preferences and their Constancy

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On recording a series of reactions to odors it was thought that a comparison with similar experiments made four or five years ago might in some measure amplify our knowledge regarding the relative constancy of affective reactions to olfactive substances. The comparison could be made only in the case of the writer, and with reference to a small number (12) of substances. Data were available, however, on affective and associative reactions to the same odors by more than sixty subjects in the former series of experiments. Moreover, other records can be referred to, particularly those by Young (3); though the object and procedure of the investigations differ in certain respects.

The odorous substances employed in 1922–1923 and in 1927 were procured from different sources, and in the case of oil (attar) of roses a synthetic product had to be substituted for the natural (Bulgarian) oil previously used. Differences in the composition of the various substances, whether due to difference of origin, or to chemical changes in the course of time, could not altogether be avoided. This particular source of error, however, did not appreciably affect the reactions, as was evidenced by a number of identical associations even in the case of essential oils notoriously variable as to properties, such as citronella oil.

In order to avoid undue complexity, affective judgments were restricted to four,

++ entirely pleasant
+− not quite pleasant
−+ not quite unpleasant
−− entirely unpleasant.

Intermediate categories, somewhat similar to those adopted by Young in 1923, had been tried; but it was found that there were certain difficulties in estimating the validity of individual judgments when more than four categories are used. For recording refinements in affective reactions, another method was found to be quite effective. This is the arrangement of odors in a series, according to the order of preference. By employing a considerable number of subjects of the same race and sex, a mean position for each odor can be determined, expressed as a percentage of pleasant reactions. For instance, male subjects gave 97.4 per cent. judgments of pleasantness to rose oil (++), 69.7 per cent. to clove oil (+−), 26.6 per cent. to amyl alcohol (−+), and 2.6 per cent. to asafœtida (−−). By employing a very large number of subjects, a fairly accurate figure for the 'standard affect' of each odor can be worked out, of course, for a homogeneous group. This method would be useful for the comparison of standard affects in various racial groups. Although originally a category 'o' for indifferent (neither pleasant, nor unpleasant) was adopted, no such judgments could be recorded in the case of subjects and odors dealt with. A considerable deviation from the standard affect may reveal a personal idiosyncrasy, each individual possessing

some characteristic set of affective judgments; an olfactory syndrome, as it may be termed.

As any shifting of the affective tone during the process of smelling, though recorded at the time, was not the special object of consideration, the judgments summarised in the accompanying tables represent the final reports given by the subject after repeated sniffing, but with due precautions to avoid olfactory fatigue, aftersmells and other disturbing factors. The eyes of the subject were shut during the experimental period, and the subject relaxed as far as possible. Associations were recorded in many of the observations; but these and other matters arising from the results are not within the scope of the present paper.

Table I shows the affective judgments given by the same subject to twelve odors in 1922–1923, and 1927. The conclusion of Young, who presented eight odors to four subjects three times a week for five consecutive weeks, that "there is very slight evidence of progressive and regressive changes in judgment" tends to be confirmed. There is evidence, however, that a relative fluctuation in the judging of odors in general does occur with fluctuating states of health. This is more particularly evident in the case of women.

TABLE I

AFFECTIVE PREFERENCES

	Amyl Alcohol	Camphor	Cassia Oil	Cedarwood Oil	Citronella Oil	Menthol	Musk	Origanum Oil	Pine Oil	Rose Oil	Sandalwood Oil	Xylol
18- 9-1922		+−		++		++	−+			++		+−
28- 9- "	−+	+−		++	+−	+−	+−					
30- 9- "		+−		++	+−	+−	+−	+−	++	++	−−	
13-10- "	−+	+−		+−	+−	+−	−+	+−	++	++	−+	−+
16-10- "		+−		++	+−	+−	+−	+−	++	++	+−	−+
9-11- "	−+	+−		++	+−	+−	+−	+−	++	++	−+	+−
4-12- "			+−									
2- 6-1923		+−	+−	++	++	+−	−+	+−	++	++	−+	
26- 4-1927	−−					+−						+−
27- 4- "	−−	+−				+−				++		
29- 4- "				++				+−	++		+−	
3- 5- "	−−	+−		++	+−	+−		+−	++	++	−−	−−
4- 5- "	−−	+−	−+	++	+−	+−	+−	++	+−	++	−+	−+
6- 5- " a	−−	+−	+−	++	+−	+−	+−	+−	+−	++	−+	−+
6- 5- " p	−−	+−	−+	++	++	+−	−+	++	++	++	−+	−+
9- 5- " a	−−	++	+−	++	++	++	++	++	++	++	+−	−+
9- 5- " p	−−	+−	+−	++	+−	+−	+−	+−	++	++	−+	−+
13- 5- " a	−+	+−	+−	++	++	+−	+−	+−	+−	++	+−	−+
13- 5- " p	−−	+−	+−	++	+−	+−	+−	+−	+−	++	+−	−+
24- 5- "	−−	+−	−+	++	+−	+−	−+	++	++	++	−+	−+
Median	−−	+−	+−	++	+−	+−	+−	+−	++	++	−+	−+

The relative constancy of likes and dislikes is demonstrated even more clearly in Table II, which shows the order of preference on the various dates and the median preferential positions for comparison. The constancy of judgment after several years' interval is very striking, and the table also reveals the idiosyncratic preference of the subject for cedarwood oil and his relative dislike of cassia oil.

TABLE II

ORDER OF PREFERENCE

	Amyl Alcohol	Camphor	Cassia Oil	Cedarwood Oil	Citronella Oil	Menthol	Musk	Origanum Oil	Pine Oil	Rose Oil	Sandalwood Oil	Xylol
Median 1922	12	7	9	2	4	6	8	5	3	1	11	10
4-5-1927	12	7	9	2	5	8	6	3	4	1	10	11
6-5- " a	12	7	9	2	5	6	8	3	4	1	10	11
6-5- " p	12	6	9	5	3	7	8	2	4	1	10	11
9-5- " a	12	6	9	2	5	4	7	3	8	1	10	11
19-5- " p	12	7	9	2	6	5	8	4	3	1	10	11
13-5- " a	12	7	10	2	3	6	8	5	4	1	9	11
13-5- " p	12	7	9	2	3	5	8	6	4	1	10	11
Median 1927	12	7	9	2	5	6	8	3	4	1	10	11
1922-1924 Males	12	8	2	5	7	3	9	6	4	1	10	11
Females	12	6	2	10	5	3	9	4	7	1	11	8
M & F	12	7	2	8	5	3	9	4	6	1	11	10

Affective reactions to a series of thirty odors observed by the writer during May, 1927 are recorded by means of the order of preference in Table III. The latter seems to confirm, as does also Table II, Young's statement that "the most variable odors are in the region of indifference while the most constant are either 'very pleasant' or 'very unpleasant.'" This requires some qualification, however. The twelve series of odor

TABLE III

ORDER OF PREFERENCE

Amyl Alcohol	29	28	28	28	28	28	29	28	28	28	28	28
Anise Oil	21	25	27	25	27	27	25	26	25	29	26	26
Benzaldehyde	13	17	16	23	22	21	21	19	22	23	27	25
Bergamot Oil	4	3	3	3	3	2	2	3	3	3	3	2
Cajeput Oil	30	30	30	30	30	30	30	30	30	30	30	30
Camphor	16	15	12	10	13	13	14	15	16	16	18	16
Caraway Oil	19	19	18	17	19	17	11	13	13	11	13	13
Cassia Oil	24	21	21	21	21	23	22	22	20	21	22	22
Cedarwood Oil	3	2	8	2	4	5	4	7	7	4	6	4
Citronella Oil	12	9	5	8	11	6	6	16	11	13	12	8
Dill Oil	18	20	17	16	20	22	17	17	14	17	14	14
Fennel Oil	28	29	29	29	29	29	28	29	29	27	29	29
Fir Oil	8	8	10	14	10	16	8	11	10	10	16	9
Juniper Oil	6	12	9	13	8	7	10	8	8	8	10	11
Menthol	17	11	14	7	9	11	9	10	12	14	9	12
Methyl Salicylate	22	23	24	22	23	24	23	27	24	26	25	27
Musk	14	16	19	11	15	14	19	21	23	22	21	20
Neroli Oil	2	5	2	4	2	3	3	2	2	2	2	3
Nutmeg Oil	23	18	20	20	18	18	20	18	18	20	19	19
Origanum Oil	7	6	4	6	7	9	12	6	6	6	5	6
Patchouly Oil	20	22	22	19	16	19	18	20	21	18	15	18
Pine Oil (Pumilio)	9	13	13	15	14	15	15	12	17	9	11	17
Pine Oil (Sylvestris)	10	7	7	12	5	8	7	5	5	5	7	5
Rose Oil	1	1	1	1	1	1	1	1	1	1	1	1
Rosemary Oil	15	14	11	9	12	12	13	14	15	15	17	15
Sandalwood Oil	25	24	23	24	24	20	24	23	19	19	20	21
Sassafras Oil	11	10	15	18	17	10	16	9	9	12	8	10
Thyme Oil	5	4	6	5	6	4	5	4	4	7	4	7
Toluol	26	26	25	26	25	25	26	24	26	24	23	23
Xylol	27	27	26	27	26	26	27	25	27	25	24	24

TABLE IV

TABLE IV

Preferred to	1 Rose Oil	2 Neroli Oil	3 Bergamot Oil	4 Cedarwood Oil	5 Thyme Oil	6 Sylvestris	7 Origanum Oil	8 Juniper Oil	9 Citronella	10 Fir Oil	11 Menthol	12 Sassafras Oil	13 Pumilio Oil	14 Rosemary Oil	15 Camphor	16 Caraway Oil	17 Dill Oil	18 Musk	19 Patchouly	20 Nutmeg Oil	21 Cassia Oil	22 Sandalwood	23 Benzaldehyde	24 Methyl Sal.	25 Toluol	26 Xylol	27 Anise Oil	28 Amyl Alcohol	29 Fennel	30 Cajeput Oil
1	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
2	1	–	7	10	11	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
3	1	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
4	1	7	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
5	1	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
6	1	5	6	5	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
7	1	5	6	5	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
8	1	5	6	5	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
9	1	5	6	5	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
10	1	5	6	5	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
11	1	5	6	5	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
12	1	5	6	5	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
13	1	5	6	5	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
14	1	5	6	5	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
15	1	5	6	5	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X	X
16	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X	X
17	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X	X
18	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X	X
19	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X	X
20	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X	X
21	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X	X
22	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X	X
23	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X	X
24	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X	X
25	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X	X
26	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X	X
27	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X	X
28	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–	X
29	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	–
30	1	5	6	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6

Times out of 12

X = preferred 12 times out of 12

preferences in Table III do show that the reactions to rose oil were always entirely pleasant and to cajeput oil entirely unpleasant, and that the adjoining odors are also fairly constant, also that the reactions to musk are variable (from 11th to 23rd place in the series of thirty). But reactions to citronella, which the subject always decidedly prefers to musk, are practically just as variable (5th to 16th place). Citronella was preferred to camphor eleven times out of twelve, the exception being camphor 15: citronella 16; yet camphor showed less variation. A finer gradation of affective reactions is made possible by the method of preference; *e.g.*, camphor and rosemary oil, both +− (not quite pleasant), fluctuate from 10th to 17th and from 10th to 18th place. Rosemary oil is preferred to camphor, and the two odors follow each other in the series. This circumstance is explained by the fact that besides α-pinene (which is present in a number of other essential oils used in these experiments) and other constituents, rosemary oil contains camphor and borneol. Another case of relative judgments dependent on chemical composition of the odorous molecule is that of toluol and xylol. The number of times each odor is preferred to each of the other odors is given in Table IV.

The order of preference as shown by this table is remarkably constant, in spite of the variable judgment given to odors like musk, benzaldehyde and citronella. In the case of citronella, the presence of constituents, agreeable and disagreeable as to smell, may partly account for relative lack of constancy in affective reaction. It cannot be confirmed that affective judgments to odors and their fluctuations are to be explained on the basis of associative tendencies. In these as well as in former experiments a distinction was made, as far as was possible, between the preference due to the odor and that due to the association. There are many cases in which odors judged pleasant have disagreeable associations and *vice versa*. Although true in some instances, fluctuations from pleasant to unpleasant, and from unpleasant to pleasant, cannot invariably be ascribed to fluctuations in association. A certain amount of variability shown may be due to similarity between odors, such as fir and pine oils.

Table I shows that the odor of cassia oil is graded as +− in 1922–1923, and as −+ on May 4th, 1927. All associations noted are extremely pleasant, and date back over thirty years, while the memory image in 1927 cannot be distinguished from that of 1922. The association (a visual image) in 1923 was different, though contiguous to the usual reaction. Fluctuations in the course of the same day were not found to be less than those of different days, nor to be due to a change in associations. They may partly be ascribed to a difference in metabolic conditions. The odor of cedarwood oil recalls a variety of associations, both pleasant and unpleasant, but the experience was always pleasant. The −− reaction to xylol of May 3rd, 1927, might possibly be ascribed to an unpleasant association.

Conclusions

Odor preferences are remarkably constant in the same individual over a considerable period of time, and apart from personal idiosyncrasies, they correspond to preferences shown by a large group of the same sex and race.

Shifting of the affective tone during the process of smelling is to a considerable extent commensurate with fluctuation recorded either within a group of subjects or during repeated observations by the same individual.

Fluctuations in affective reactions to an odor cannot be wholly attributed to associative tendencies, but more to the nature of the odorous substances and to the metabolism of the subject.

The arrangement of odors in order of preference is a convenient method by which to compare the affective experiences of individuals and of groups.

It is desirable and necessary that the range of odors and the number of subjects should be as large as possible; but numerous observations by a single individual are also essential.

The same associations can be evoked by unlike odors of similar constitution but occupying different positions on the affective scale.

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A Note on the Spearman-Brown Formula

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In Lanier's recent article in this Journal (1) on the empirical verification of the Spearman-Brown formula he is concerned, among other things, with a possible relation between the number of subjects who take a mental test and the reliability coefficient of that test. When I came to this part of his article I was puzzled because the reliability coefficient of a test is not a function of the number of cases on which the reliability is determined. This particular part of Lanier's study seems to have its provocation in a serious misunderstanding of Miss Gordon's interesting experiments (2) which have been interpreted by Kelley in terms of the Spearman-Brown formula (3). Reading Lanier's article, especially his conclusions, more closely makes it appear certain that he has misinterpreted Miss Gordon's experiments.

Let us start with Lanier's own statement of one of his purposes. "A fourth aspect of this study has been to determine the predictions made with the formula (Spearman-Brown) when the increase is in the number of subjects rather than in the length of the test." It surely would be startling and disconcerting if the reliability of a test were a function of the number of subjects used in determining its self-correlation. If such a relation should exist it would be possible to boost the reliability of a test simply by giving it to more people! As is well known, the heterogeneity of a group affects the reliability coefficient, but as far as I am aware no one has ever claimed that the reliability coefficient is a function of the number of cases.

Lanier's starting point for his experiments on the number of cases in relation to the reliability coefficient seems to be Miss Gordon's experiments which he summarizes in some detail (pp. 77–78). Describing Kelley's analysis of the Gordon experiment he says: "There is, then, moderately close agreement between the increase in reliability by actually increasing the number of judges and that predicted by the formula." As a matter of fact, the agreement between Gordon's data and the Spearman-Brown formula is quite satisfactory. And then: "*The data used in the present study were treated in this same manner and no such conformity was found* (italics mine). One cannot conclude, then, that there is, for all kinds of material a close relationship between the number of subjects and the amount of reliability to be expected." Here it is evident that the Gordon experiments are interpreted as though they indicated a relation between reliability and the number of subjects who take a test! The Gordon weight experiments have nothing in common with Lanier's experiments on the number of subjects. The disparity should be made clear.

Miss Gordon's experiments are briefly as follows. A series of weights are to be placed in rank order by each subject. The true rank order is of course known to the experimenter. Now, if we pool the rank orders of five subjects we shall have a certain correlation between this obtained rank order and the true rank order. If we pool the rank orders of ten subjects we shall expect a higher correlation with the true rank order. What is the functional relation between the number of subjects whose judgments are

pooled and the truth? This is a clever experiment. It is a simple situation in which truth is objectively known and we want to determine how the number of judges, pooling their estimates, affects proximity to truth. Kelley showed that the Spearman-Brown formula applied fairly well to Miss Gordon's data. At this point Lanier misinterprets the nature of the experiment. The number of judges in Miss Gordon's experiment corresponds to the number of items in a test. The test score is a pooled result of a number of test items and just so Miss Gordon's 'test' is the pooled effect of a number of judges. This comparison can be carried further. Just as the reliability of a test is usually raised by increasing the number of test items, so the reliability of a pooled rank order is raised by increasing the number of judges making it.

In his final conclusion Lanier says: "There appears no ground for the assumption of a general increase in reliability proportionate to the increase in the number of subjects, such as the prediction formula gives." Nobody has made that assumption and the prediction formula does not 'give' or imply any such relation whatever. It is clear therefore that Lanier has misinterpreted both the meaning of the prediction formula with reference to the number of cases about which it says nothing and Miss Gordon's ingenious experiments which also imply nothing that corresponds to the number of subjects who take a mental test.

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